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THE NATURE OF LIVING MATTER

THE NATURE OF LIVING MATTER

BY

LANCELOT HOGBEN

Professor of Social Biology in the University of London

The mind that needs to know all things must needs at last come to know its own limits, even its own nullity, beyond a certain point. D. H. LAWRENCE

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To
BERTRAND RUSSELL

FOREWORD

In the summer of 1929 I was asked to speak for thirty-five minutes in a symposium on the Nature of Life arranged by the officers of the Physiological Section of the British Association. I soon discovered that there are many ways of filling up thirty-five minutes devoted to the consideration of so formidable a topic. Eventually I decided to make my contribution in the form which appears almost unchanged in the fourth essay of this book. By that time I had written a volume without intending to break my silence before attaining my sixtieth year. With the insertion of some material to elaborate the point of view I had developed this collection might be described as the rejected addresses. I owe to my friend Mr Sewell, who has adopted the same standpoint in a criticism of æsthetics in course of preparation, the suggestion of the word *public* in contradistinction to the *external* world of Professor Eddington.

Four contributors to the Symposium on the Nature of Life, General Smuts, Dr Haldane, Dr Wildon Carr and Professor Eddington, had already published their philosophical views in book form. General Smuts and Dr Haldane courteously wrote to me, expressing the hope that I would criticize their views destructively. I found the time at my disposal insufficient for stating my own point of view. I have responded to their invitation in these essays. If

certain passages seem to some of my readers unduly polemical, I have the assurance that my fellow-contributors will regard this collection as the continuation of what was a very friendly argument.

I wish to acknowledge my indebtedness to my friend Professor Levy for criticism and assistance in seeing this book through the press.

L. T. H.

Cape Town,
April, 1930.

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PART I

VITALISM AND MECHANISM

SUMMARY

An uneasy recognition of the conflict between science and common sense in our generation has rekindled interest in the relation of science to moral philosophy. In this awakening the physicists have assumed the leading part. It will not be possible to predict the outcome, until the contribution of contemporary biology to natural philosophy is taken into consideration. Some writers have expressed the hope that the influence of biological concepts may assist to a reconciliation of the claims of natural science and moral philosophy. This hope is based on a failure to recognize that modern experimental biology is an ethically neutral body of enquiry. The merits of a mechanistic or vitalistic outlook in biology have been too often discussed from an ontological rather than an epistemological standpoint. Our estimate of the influence of biological concepts on the future of natural philosophy must be guided by a recognition of the essential similarity of method in biology and physics. This similarity is nowhere more evident than in those branches of physiology which lie most conspicuously outside the realm of applicability of physico-chemical hypotheses. Traditional mechanistic physiology has accepted the Cartesian dualism of mind and matter. The modern physiology of the conditioned reflex has undermined the distinction between reflex and voluntary behaviour. There is thus no nicely defined boundary at which physiology ends and philosophy begins. Biology is annexing regions of enquiry which have hitherto remained the province of moral philosophy. As a concept of biology *Mind* is replaced by *Behaviour*. Since modern biology claims to interpret the characteristics of conscious behaviour as properties of physical objects, the advance of biological science cannot be expected to reinforce the claims of moral philosophy. How far it is possible to reduce the interpretation of behaviour to purely physico-chemical hypotheses, we have no means of predicting. At present we can foresee no limit to progress in that direction. The significant issue is not the completeness of the mechanistic solution, but whether there exists any definable method of arriving at a more complete solution than the mechanistic outlook permits.

"It is our happiness to live in one of those eventful periods of intellectual and moral history when the fast-closed gates of discovery and reform stand open at their widest. How long these good days may last we cannot tell. It may be that the increasing power and range of scientific method with its stringency of argument and constant check of fact may start the world in a more steady and continuous course of progress than it has moved on heretofore. But if history is to repeat itself according to precedent, we must look forward to stiffer, duller ages of traditionalists and commentators, when the great thinkers of our time will be appealed to by men who slavishly accept their tenets, yet cannot or dare not follow their methods through better evidence to higher ends. In either case it is for those among us whose minds are set on the advancement of civilization to make the most of present opportunities that, even when in future years progress is arrested, it may be arrested at a higher level."

Tylor's *Primitive Culture*

INTRODUCTION

§ 1

No one who is familiar with contemporary thought can have failed to recognize two characteristics which have emerged into prominence during the past two decades. With increasing elaboration of its logical technique, science has been brought into apparently irreconcilable conflict with common sense. The result is that scientists, uneasy in the realization of this conflict, are seeking to establish a new working relation between science and philosophy. This *rapprochement* has been brought about especially through recent progress in physics. It will not be possible to predict its outcome so long as the physicist claims to speak for science as a whole. In this introductory essay I propose to discuss in a somewhat discursive and preliminary way how far the conflict between science and common sense is apparent rather than real, and to indicate the special need for reviewing the progress of modern biology in its philosophical bearings.

At the present time few biologists are anxious to court publicity in the field of philosophic controversy. Those who do so are rarely numbered among the ranks of those who are still actively contributing to contemporary progress in biological enquiry. Those who are actively contributing to the advancement of biological knowledge show little disposition to commit themselves to far-reaching generalizations. There has emerged from the morass of speculation associated with the rise of the evolutionary hypothesis a recognition of the paramount importance of painstaking quantitative study of limited aspects of vital phenomena. This attitude is a salutary one. It does not signify that

biology is passing through a phase of stagnation. On the contrary current biological discoveries contain the germ of philosophical issues which may prove to be as revolutionary as Relativity and as repugnant to common sense. In the essays which follow I shall confine myself to accredited experimental data. I do not pretend that all or even a majority of biologists will agree with my interpretation of their philosophic significance.

There is no novelty in asserting the need for incorporating the contribution of biological science in a modern philosophical outlook. Herbert Spencer and the Evolutionists prepared the ground fifty years ago; but they failed to lay emphasis on the methodological aspect of biological enquiry. The methods and not the results of biological science are specially significant to philosophical discussion. In putting forward my own views upon the nature of life, it is not the results of biological enquiry, but the methods which I propose to discuss in the first series of essays in this volume. In contrasting the methods and concepts of physical and biological science, I shall sometimes draw inferences which will not commend themselves to the judgment of biologists for whose contributions I entertain a lively respect. I shall not be surprised to be told that my forecast of the outcome of biological enquiry is pretentious, and that my philosophical conclusions are in conflict with common sense.

At an early age I abandoned the conviction that scientific hypotheses must conform to the requirements of common sense. When I was a boy, there used to be in Portsmouth, the town of my nativity, a public figure by name Ebenezer Breach. Mr Breach was a philosopher. To be precise he described himself as " Natural Astronomer and Poet ". In

that he belittled his gifts. Of his poetry I shall say nothing, save that he stated the qualification " poet by Royal Patronage " in his fascinating brochure *Twenty Reasons against Newtonianism or The Universal Challenge to Unnatural Science.* This was sold for the modest price of twopence sterling. As his contribution to modern thought may be unfamiliar to many cultivated people who were not born in Portsmouth, I propose to quote the first of his twenty reasons as representative of the system he develops :

> " Because the earth has no axis, therefore nothing on which to revolve, an imaginary mathematical line is substituted. But no solid body could revolve on an imaginary axis or line. It is an imaginary cause which can only produce an imaginary effect, so all that follows the cause must be imaginary. If anything be placed on the top of a revolving body it will fly off at a tangent."

From this you might infer, wrongly it happens, that Mr Ebenezer Breach earned a comfortable livelihood as Regius Professor of Moral Philosophy in an authentic University. He had in fact chosen to bear witness to the hope that was in him by the only alternative which a harsher economic destiny had imposed. Every Saturday night he addressed a handful of half-intoxicated seamen, tired commercial travellers, adventurous nursery maids and irreverent pupil teachers foregathered on the sea-front. There he occasionally succeeded in selling a copy of the *Twenty Reasons,* and beyond this obtained, as far as I am aware, no reward in the life that now is. In spite of his erudition and distinction of person, Mr. Breach, the prophet of common sense, did not make many converts. He was less successful in his popular appeal than an evangelical competitor who used to minister to Portsmouth beach before a banner whose legend stated, " the wages of sin is death ". This banner

I can still recall as, in its way, a work of art. On the foreground were displayed the theatre, race-course, public-house, dancing saloon and gaming tables along the edge of a precipice over which poor folk in a semi-incandescent condition were tumbling into a lake of brimstone and fire. It invariably drew a large crowd. I had early imbibed the notion that science like Sunday travelling, whist and dramatic entertainment is worldly, so that the gospel of Mr Breach, who condemned science on account of its essential unworldliness, presented a new and arresting point of view, On the whole the inhabitants of Portsmouth were more interested in their souls and what would become of them after death. Mr Breach had another competitor with more peculiar views about the soul and about life. As far as I can remember he held that the brain secretes consciousness in much the same way as the liver secretes bile, and he asserted that the soul was the shadow cast by the machine. My nurse held very definite views about his domestic life. He was a materialist, and in all probability a polygamist, if not worse. Mr Breach who was a bachelor, the evangelist who was certainly not a polygamist, and the Secularist who was undoubtedly a bad man all agreed in one particular. Each believed that the gospel he proclaimed was common sense.

Of the Flat Earth faith Mr Ebenezer Breach is the only Confessor and, financially speaking, Martyr I have been privileged to encounter. I cherish the recollection of his secular ministrations for a reason which is eminently relevant to everything which I propose to say about the bearing of current biological concepts on philosophical discussion. At an age when, to my way of thinking, Punch and Judy were the only serious rivals to the magnetism of his stupendous

intellectual gifts Mr Breach stands out in the sharp relief of retrospect as the Forerunner of the coming conflict between science and common sense. I have already remarked that the uneasy recognition that science conflicts with common sense has been the keynote of philosophical controversy during the past decade. Curiously enough some scientists seem to regard this as a grave disability on the part of science. They feel compelled in consequence to adopt an apologetic attitude to the claims of traditional philosophy. Perhaps this is because the protagonists of science in the nineteenth century made it their proud boast that science is nothing more than organized common sense. They therefore felt that they had the man in the crowd on their side. Even Herbert Spencer, prophet of evolution, when evolution was still a subversive doctrine, could soberly declare that " the ultimate truth of a proposition is the inconceivableness of its negation ".

Neitzsche includes this quotation in the *Will to Power* as one of his " inscriptions over the porch of a modern lunatic asylum ". It is only necessary to mention the word Relativity to indicate how impossible it would be for a natural philosopher to express himself in similar terms to-day. The situation which has been created by progress in modern physics is not without parallel in human history. It is true that the new theories have employed an immensely elaborate and difficult logical technique. How far they can be simplified it is at present impossible to predict. Newton's fluxions were unfamiliar to his contemporaries. The author of the *Principia* devoted a good deal of time to a geometrical presentation of his ideas, in order to make them accessible to his generation. For more than a century after Newton's death the calculus remained a preserve for

mathematical specialists. To-day a knowledge of the calculus requisite to an elementary understanding of the theory of elliptical orbits lies within the scope of the first year's work at a university, if it has not been acquired in the higher forms of a good school. It is conceivable that the mathematical development of modern physical theories will be simplified in the course of time. In that sense the esoteric stage through which physics is now passing may be a temporary phase. The essential feature of the conflict between common sense and physical science in this generation lies in the unfamiliarity of the new concepts. The conflict between common sense and the new biological concepts shares the same characteristic.

In Bernard Shaw's *St Joan*, La Tremouille asks: "Who the deuce was Pythagoras?" "A sage", replies the Archbishop, "who held that the earth is round and that it moves round the sun." "What an utter fool", says La Tremouille, "couldn't he use his eyes?" La Tremouille here calls attention to a fact that was overlooked by Herbert Spencer, by Mr Ebenezer Breach and by those Relativist philosophers, who, being unable to convince the man in the crowd, indulge in the luxury of wondering whether the claims of scientific method have been pushed too far. Common sense is another name for what good citizens are prepared to accept without argument. Scientific ideas only conflict with common sense so long as they are still new and unfamiliar. Mr Breach was in advance of his time in daring to criticize the Newtonian system. He was behind his time in thinking that Newton's position could be assailed successfully with the weapons of common sense. The essential rightness of the Newtonian system had already become incorporated in British middle-class respectability. To the

rising generation suckled on Mr Wells' *Outlines* evolution is common sense. Two generations have elapsed since, as La Tremouille would say, any fool who used his eyes could see that a bishop was a product of special creation. The man in the crowd has no clearer notion of the logical status of the doctrine of descent than had his grandfathers who implicitly accepted the story of the Fall.

The phenomenal success of those who set out to popularize the Theory of Evolution makes it easy to overlook the circumstance that evolution was wholly repugnant to common sense within the memory of those who are still living. The outburst of public controversy which greeted its announcement has no parallel in this generation. In consequence its impact upon traditional philosophy has been far less apparent than its influence upon religious dogma and social theory. The younger generation of biologists cannot recapture the first fine raptures of enthusiasm which their elders experienced. The prevailing attitude is to welcome a return to the complacent dualism of pre-Darwinian days, when scientists did not meddle with philosophy and metaphysicians conceded to scientists the right to go to the devil in their own way. Although this view is widely held, I do not believe that the philosophical implications of evolution have ever been thoroughly explored; or that it was possible to do so, while the study of animal behaviour was still dominated by the language of introspective psychology. By explaining the secular origin of philosophers Darwin bequeathed to us the task of elucidating the anatomy of philosophy.

In the opening years of the present century, science had already lost that truculence which one associates with the generation of Huxley and Tyndall. It had surrendered

its tradition of fearlessness and candour. Academic philosophy, liberal theology and utilitarian science went their placid ways without mutual interference. Bergson, a philosopher more widely known than Mr Ebenezer Breach, had cast a pebble of *belles lettres* into the mill-pond of compromise. The indifference with which it was greeted by those engaged in the task of placing the evolutionary problem upon a secure foundation of experimental data is a measure of the esteem which they entertained for it. It has been interpreted as assent by some contemporary writers who are not themselves biologists. In *Science and the Modern World* Dr Whitehead even speaks of Bergson's "instinctive grasp of modern biology". A modern biologist engaged in the study of behaviour would refer with greater caution to Madame Blavatsky's instinctive grasp of modern astronomy or Mahatma Gandhi's instinctive grasp of modern economics. He would regard the instinctive grasp of any branch of scientific knowledge with more suspicion than approval. No biologist has undertaken the task of examining the philosophical implications of Darwin's doctrine in the light of contemporary progress in the experimental analysis of living matter.

§ 2

Under the influence of Hegel, academic philosophy left the scientist to his own devices. To-day the physicist has again driven the idealist philosopher out of his retreat. He has compelled him to take account of a conceptual world which we all recognize whenever we consult a railway time-table or book a passage in an ocean liner. Secure in the prospect of fresh philosophical victories, the astronomer surveys the world with a blind eye to the micro-

scope, and magnanimously dictates the new territorial frontiers of science and moral philosophy to the advantage of the latter. It has been customary in the past, and therefore common sense, to assume that the issues with which moral philosophy deals are more fundamental than those which fall within the scope of natural science. It is an assumption which, whatever its meaning, does not hamper the advance of pure physics; but the biologist is not bound to accept this convention when it restricts his own field of enquiry. A philosopher is a particular kind of organism. Philosophy itself might therefore be regarded as an aspect of the behaviour of a piece of living matter. The study of the properties of living matter is the province of the biologist. From this point of view the study of biology is more fundamental than the pursuit of moral philosophy.

The physicist brings to the discussion of philosophy the discipline of an older branch of enquiry with a more elaborate logical technique than that of biology. His claim to speak for the whole field of science should be scrutinized with a critical eye. I am sure that Professor Eddington will agree with me, when I say that the biologist has a specific contribution to make to what he has aptly called the " world symposium ". I am also confident that many biologists will agree with me, when I state that the contributions of the Relativist philosophers rarely display a profound understanding of the kind of problems biologists are now attempting to solve, and the way in which the modern biologist sets about his task. A quotation from Mr Sullivan's *Bases of Modern Science*, a stimulating and provocative book, will illustrate my meaning. Mr Sullivan, whose physics I do not venture to criticize, states: " The primary concepts in terms of which the science of physics is con-

structed . . . have to be supplemented by others in the science of chemistry, and for the sciences of life and mind, are so far from being sufficient, that they have hardly yet been found to be relevant." Half a century has passed since the concept of chemical affinity was annexed by thermodynamics, and the most conservative physiologist could hardly refrain from ridiculing the latter part of this quotation. Professor Eddington himself has adopted the " Principle of Indeterminacy " as an *ad hoc* hypothesis in a limited field of enquiry. From it he proceeds to draw conclusions about human responsibility and the doctrine of free will. These are topics which lie nearer to the province of biology than physics. It would be well to await the verdict of biological science before accepting inferences of so far reaching a character as those which Professor Eddington has advanced.

By emphasizing the conflict between science and common sense Relativity has engendered a new interest in the relation of science to moral philosophy. To view that relation in its proper perspective the concepts of modern biology must supplement the concepts of modern physics. I am not suggesting that this need is overlooked by those who are not biologists. Dr Whitehead has gone so far as to advocate replacing the traditional physical idea of matter by the biological concept of organism, or as a modern biologist might prefer to say, behaviour. When he expresses the hope that this will assist to " end the divorce of science from the affirmations of æsthetic and ethical experiences ", it is clear that his conception of the nature of biological enquiry dates from Herbert Spencer and differs from that which contemporary biologists would generally be willing to accept. Owing to the separation of descriptive from

experimental biology, a separation for which the evolutionists were pre-eminently to blame, a well-informed interest in the study of living matter is more rare among physicists of our period than it was in the days of Robert Hooke and Boyle or of Euler, Lavoisier, and Laplace. I am convinced that very few scientists who are not biologists—perhaps no professional philosophers—possess a clear notion of the way in which the modern experimental biologist approaches the study of the organism and the results at which he aims.

I have already suggested that there are special reasons why the concepts of biology stand in a more intimate relation to the scope of moral philosophy than do those of physical science in the restricted sense. It is difficult to define the meaning of philosophy without implying a particular point of view about the limitations of human knowledge. There are as many different definitions of philosophy as there are different schools of philosophical opinion. From the point of view of the materialist a Hegelian is a sea lawyer. From the point of view of the subjective idealist a materialist is not a philosopher at all. If there is anything which all the two and seventy jarring sects would agree to regard as a problem of philosophy, it is the Nature of Life. If we are to avoid making any unjustifiable assumptions about the nature of knowledge, we must for the present define a philosophical discussion of the Nature of Life as the most comprehensive treatment of the problem. It does not necessarily follow that there is any essential difference between a scientific and a philosophic enquiry in this sense.

When people first hear their own voices recorded by a gramophone, it is well known that they are often—like myself—a little humiliated, and generally somewhat sur-

prised. I once had occasion to witness an instructive incident which occurred in the phonetics department of the University of Cape Town. A gramophone record of three men engaged in a conversation was prepared. None of the three participants had previously listened to a record of his own voice. When the record was completed each man agreed that the voices of the other two were faithfully recorded. Each man denied that his own voice had any semblance to its representation by the recording instrument. This simple experiment in human behaviour illustrates what I shall later call the distinction between the *private worlds* and the *public world*. It also illustrates a fundamental divergence of outlook which distinguishes two tendencies in philosophical discussion, and makes it difficult to give any definition of philosophy satisfactory to all parties. One school of philosophers defines a good record as a record which on the whole faithfully conveys the impression of human voices. The philosopher of the opposing school feels that it ought to be possible to manufacture a record which will faithfully represent the voice of his opponent, while at the same time registering his own voice as he hears it himself, when he is speaking, and would prefer other people to hear it.

This distinction has an interesting history which will be discussed in the third series of essays in this volume. Greek speculative philosophy had its first beginnings in a secular curiosity about Nature. Science and philosophy were thus one and the same thing to Thales, to Empedocles or to Democritus. In Greek thought speculation was not sufficiently disciplined by sustained observation of Nature. For that reason it gave birth to innumerable conflicting hypotheses which could never be made the subject of decisive tests. Out of this confusion of conflicting ideas was born a

reaction against science. Philosophy turned from the slow and tedious task of examining the actual world to the more facile and pretentious pursuit of an ideal world. In the person of Plato it forfeited its secular temper. Science was introduced into modern Europe by the Arabs, who assimilated the secular curiosity of the Greeks. Ecclesiasticism seized upon the speculations of the later Greek philosophers to provide a rational basis for theological dogma. Since mediæval times scientists have submitted to an arrangement which gives to those who have not studied Nature the right to supervise the logical status of their conclusions. The stability of this arrangement has been maintained by the circumstance that human beings are far more interested in themselves than in any other material objects. Greek materialism declined, because it could not satisfy man's curiosity about himself. The success of its rival was not due to its ability to settle the problems of human nature and social conduct. It succeeded because human nature demands a forum for the ventilation of its grievances. Science has been most successful in the past in dealing with inanimate things. Only in comparatively recent times has the phenomenal success of scientific method, fortified by the secular influence of Darwin's teaching, encouraged the belief that it might be applied to the study of man's behaviour and social organization. The belief that a philosophical discussion of the Nature of Life lies beyond the province of the biologist is due to centuries of subservience to a tradition which has identified philosophy with the interests of statesmanship and ecclesiasticism. If the method of science is applicable to the study of how statesmen and theologians behave, it is legitimate to undertake a discussion of the nature of life without assuming that the

biological standpoint must be reinforced by the discipline of scholastic philosophy.

There is a further assumption which we need not make in our enquiries into the Nature of Life. We need not presume like Socrates that all questions about life are permissible. A proper respect for our own limitations is as essential to philosophy as to sanity and modesty in everyday life. It is only possible to formulate questions in the right way when we already have at our disposal a good deal of information relevant to the correct answer. This was not recognized by the materialists of the nineteenth century when they attempted to give a common-sense solution of the Riddle of Life. To-day it is customary to refer to materialism as an exploded fallacy. If instead of looking at the way in which the materialist attempted to answer the man in the crowd, we examine the way in which he attempted to answer the questions which he himself propounded, the explosion of the fallacy is not so encouraging to traditional beliefs. The term materialism, when it is not employed like Bolshevism as a term of abuse, is loosely applied to a constellation of beliefs, some of which concern the Nature of Life and some of which concern the Nature of Knowledge. In the latter sense materialism implies the conviction that the only genuine knowledge is that which can be gained by pursuing the method devised by scientists for the study of what are ordinarily called material objects. If this conviction is carried to its logical conclusion, a discussion of the Nature of Life in language which is intelligible to the audience of Mr Breach or his Secularist competitor is impossible. The materialist who attempts a common-sense solution of the Riddle of Life is inconsistent with his materialism. It is his inconsistency and not his materi-

alism which is an exploded fallacy. Common-sense materialism, the materialism which is an exploded fallacy to-day, was based on the belief that a plain answer to a plain question is the inalienable birthright of the plain man. Secularist rationalism was the offspring of Protestant democracy. Protestant democracy is suspicious of the expert, who is the person who knows that there is a technique of asking questions in the right way as well as a technique of answering them in the right way. Perhaps Xanthippe, who has become the symbol of a nagging wife, realized this profound truth more clearly than Socrates. Perhaps her short way with introspective philosophers was based on a considered recognition of human frailty, and experience of children.

An intelligent child of three once asked me to tell her the colour of Wednesday. To the more sophisticated adult the question is ridiculous, though I suppose the theosophist would regard it as permissible. Metaphorically speaking, the habit of asking the colour of Wednesday is not exclusively confined to children. Thousands of years ago human beings began to associate particular sounds with objects around them, so that these sounds became signals for activities. These activities became increasingly more complex as articulate speech became more elaborate. Gradually human beings ceased to employ a separate symbol for every object around them. They began to condense and economize, abstracting separate properties. It became no longer necessary to have separate words for white cow, black cow, white horse and black horse. In effecting this economy it was inevitable that new words with no clear relation to experience were often invented. Common language the world over is burdened with words which effect no economy of discourse. Anyone who has not realized

this may perform the simple experiment of asking six educated people to define in writing on a folded slip of paper the meaning of the word *sincerity*. With the coming of civilization man invented a new form of symbolism, the language of science. In spite of immense social inertia this symbolism has become more and more important, because of the tremendous power for controlling nature which it has given us. In the invention of this new language not only sustained observation of nature, but active interference with nature, or experiment, is enlisted in the process of abstraction.

Because common language and the language of science are not the same thing, there never can be a plain answer for the plain question of the man in the crowd. There can only be a familiar one. In any restricted field of scientific enquiry confusion of thought is avoided by the introduction of new symbols to denote new experience, or a preliminary re-definition of old symbols, if these are employed. So long as the chemist is only concerned with the reducing power of a particular sugar, it is sufficient for him to describe it as dextrose. When he directs his attention to the optical properties of the sugars he finds this symbol no longer adequate to define a homogeneous class, and distinguishes between α dextrose, β dextrose and so forth. When scientific hypothesis so broadens its channels as to merge into the general current of human thought, the scientist finds himself dealing with matters for which there already exists a vocabulary, but one that has none of the precision of scientific nomenclature, one called into being by an approach to experience which has none of the disciplined restraint which scientific method imposes. That is why the practising scientist is sometimes

compelled to treat the conundrums of humanistic philosophers like the question of the child who wanted to know the colour of Wednesday. Concerning those things about which we talk most our language is apt to be least definite.

§ 3

One of the things about which we talk most is life itself. A discussion of the Nature of Life presupposes that we mean something quite definite, when we use the term life. The familiar lines of Mr. Belloc suggest a helpful analogy to illustrate the nature of a scientific definition :

> " Here you may put with critical felicity
> The following question, ' What is Electricity ? '
> ' Molecular activity,' say some.
> Others remain silent or are dumb."

What is electricity ?—is a plain question. It is only possible to give it an intelligible answer when we translate it into the form, what conditions determine electrical phenomena ? A scientific concept is a label for a class of properties which can be investigated scientifically. Though this happens to be a cardinal doctrine of modern logicians, it is also a commonplace of scientific thought, when undisturbed by ulterior considerations. It is a commonplace which is constantly overlooked by biologists as well as laymen in a discussion concerning the nature of life. The temptation to overlook it is assisted by the custom of spelling nature and life with capital letters, a practice to which what the Melanesians call *mana* adheres. The only intelligible significance of the word Life in scientific discussion is to denote collectively the properties of living[1] things.

The word life is variously employed in common language.

[1] An examination of the precise significance of this adjective is undertaken in the essay on the Nature of Life, p. 80 et seq.

To Mr Mantalini life is one demmed thing after another. Every biological student is familiar with the experiment of removing a frog's heart from its body, maintaining its beat by perfusing it with a suitable saline medium, arresting its rhythm and restarting it by changing the constituents of the medium. This can be performed repeatedly for many hours after the owner of the heart is, legally speaking, dead. The layman confronted with this commonplace of the laboratory invariably asks with some show of bewilderment, " Is it alive ? " It is extremely difficult to answer him in words he will understand. He has been accustomed to think of an organism as a whole, just as we think of solid matter as a whole. Unaided common sense does not easily grasp the notion that the frog's heart displays the characteristic properties of living matter, after the frog, considered as a whole, has ceased to display those characteristics of living matter which we associate with whole frogs, when we say that they are alive.

In biological discussion the nature of life can only be understood to mean the characteristic properties of living things, how they are related to one another and to the properties of non-living matter, how they have come into being. To those who are accustomed to thinking in abstract nouns and capital letters this way of defining life will seem rather like the well-known definition of an archdeacon as a man who discharges archidiaconal functions; but if life is only a convenient label for the properties of living matter, we have foreshadowed an important conclusion. Those who declare that materialism is an exploded fallacy are usually those who deplore the judicial separation of science and moral philosophy. If they entertain the hope that biology is likely to effect a restitution of conjugal

rights, they evidently imply that life to the biologist means something more than the properties of living matter. They assume that a biological concept of life contains other implications of its use in common language.

The source of this confusion is easy to understand. The biologist can no more avoid using the word life than the physicist can avoid using the word matter in a loose and arbitrary sense in everyday conversation. Whatever meaning the biologist may attach to the term life, when he is exercising his domestic and political activities, there is only one legitimate manner in which he can employ it in his capacity as a scientist. Much discussion between the opposing schools of vitalists and mechanists is utterly barren, because this fundamental issue is not clearly defined at the outset. The vitalist can legitimately attack the mechanist by pointing out that living things have characteristic properties other than those which the mechanist attempts to analyse. If he does so, he must specify what such properties are. In the laboratory the biologist carries out his work on the same lines, whether he calls himself a vitalist or a mechanist. On the platform he may, and frequently does, overlook this. The layman may thus acquire a disproportionate estimate of the extent to which biologists differ among themselves about fundamental issues.

That biologists are still less unanimous than chemists in the hope of resolving, in more universal terms, concepts traditionally restricted to their own fields of enquiry, may be attributed to the complexity of their subject matter. Biology is a younger science, and a vast amount of purely descriptive work was necessary, before it was possible to formulate the mechanical problems which living matter presents. This task requiring considerable specialization in

the descriptive study of the exclusively geometrical aspects of the configuration of living systems unhappily became divorced from the more fundamental issue of biological enquiry. The physical analysis of the properties of living matter is a problem necessarily spatio-temporal in its extension, experimental in its method, and quantitative in its grammar. The spectacular success of evolutionary speculation during the nineteenth century preceded the birth of quantitative and experimental researches on inheritance and variation, giving descriptive biology a reflected glory on account of the far-reaching cosmological consequences of the doctrine of descent.

By encouraging the hope of reconstructing the pedigree of mankind, Natural Selection widened the gulf between descriptive and experimental enquiry; and provided a satisfactory modus vivendi for two diverging and independ nt schools of research. Anatomy claimed the relation of one type of living being to another. Physiology concerned itself with the relation of living matter to inanimate objects. During the present generation evolutionary problems have emerged to the forefront of experimental enquiry. Heredity and variation are no longer axioms with which the taxidermist and the osteologist can conjure unchallenged. Experimental biologists are grateful to those who have compiled the Who's Who of the Animal Kingdom. They refuse to concede that the execution of this task implies a profound understanding of the principles of political economy. Naturally the anatomist and the field naturalist view the change with a jealous and suspicious eye.

Biologists agree among themselves in recognizing that the approach to the organism as a physical object has led to many valuable discoveries; and that the application of physical methods to the study of the organism permits us

to make many predictions about the behaviour of living systems with as much confidence as we have in predicting other secular events. In so far as recent investigation has probed into phenomena which it has been customary to place beyond the limit of applicability of physical methods and concepts to the analysis of the properties of living matter, it is not surprising that many biologists have failed to take stock of the situation. They may simply deny that certain aspects of the behaviour of organisms can be treated successfully by the traditional methods of experimental physiology. If they do, it should be sufficient to set forth the new evidence at our disposal. When they go further and assert that certain characteristics of living things properly belong to the sphere of traditional philosophy, it is permissible to entertain the suspicion that they share the all too human desire to be *certain* rather than to *know*.

The chief source of disagreement between different schools of opinion in any discussion of the Nature of Life arises from the difficulty of defining another concept which is intimately connected, but not necessarily co-extensive with, that of life itself. It has been customary in the past to assume that the concept of *consciousness* defines a field in which the methods of experimental physiology break down and require to be supplemented by the method of introspection. In so far as it bears on the Nature of Life this implies the possibility of identifying and specifying in living systems characteristics to which the term consciousness directs attention. It is impossible to avoid disagreement in connexion with this concept without recognizing a fruitful source of confusion. The statement " I (N or M) am a conscious being " has a formal relation to the statement " All men are conscious beings " like the analogous statement " Mr. Bertrand Russell is a conscious being ", so

long as it is understood that I and Mr Bertrand Russell are both single valued and members of the class " men ". From this it follows that any implication of the first proposition which is not implicit in the second defies logical analysis and therefore eludes philosophical enquiry. For the purpose of philosophical discussion " I am a conscious being " contains nothing that is not implied by saying that " all men are conscious beings ". I shall use the term *public* to signify this way of looking at the concept of consciousness. Any residuum of the first proposition which cannot be formally identified with the third and shown like it to be included in the second and more general proposition is a *private* affair of the individual. If we find that modern physiology has undertaken to investigate those characteristics of the behaviour of living systems associated with the term consciousness in its public sense, a new horizon of philosophical discussion is unfolded. If physiology is more successful than introspective philosophy in defining predictable conclusions about living behaviour, we have no need to go outside the data of physiology for the materials of a comprehensive discussion of the Nature of Life. A philosophical discussion of the Nature of Life will only be more comprehensive than a biological discussion of the Nature of Life in the sense that more attention will be paid to the methods of enquiry adopted.

Between two extreme schools of opinion existing at the present day the issue, in so far as it is a tangible one to the practising biologist, is thus defined by Dr Haldane in his recent Gifford Lectures.

> " We can of course leave the characteristic peculiarities of conscious behaviour out of account, and regard persons from a purely physical and chemical point, as weighing so much,

> as yielding certain amounts of various proteins and other chemical substances, distributed in a certain way, and as in various ways continually converting potential into kinetic energy. This mode of regarding persons is of great practical use for engineering and other purposes, *but tells us nothing, however far we may extend it, regarding the distinctive* characters of conscious behaviour . . ." (italics inserted).

In this passage Dr Haldane is perfectly definite in stating where, as he believes, the methods of traditional physiology cease to be applicable. It is peculiarly felicitous that he uses the term conscious behaviour rather than consciousness in this connexion. If we find reason to believe that " conscious behaviour " can be analysed with reference to a space-time framework by the methods of physical science, Dr Haldane's attack on the mechanistic position falls to the ground except in so far as he can refuse to capitulate until the problem has been reduced to a question of pure physical chemistry. In the succeeding essay on The Mechanization of Consciousness I shall endeavour to show that in our generation the work of Pavlov's school has successfully tackled, for the first time in history, the problem of what Dr Haldane calls " conscious behaviour " in non-teleological terms. It has reduced it to the investigation of the conditions under which new reflex systems are brought into being.

In *Science and the Modern World* Professor Whitehead states that the " effect of physiology " on philosophical discussion has been " to put mind back into nature ". I presume that he is referring to the traditional distinction between reflex activity and voluntary behaviour. It is true that physiology has accepted this distinction which it inherited from the dualism of Descartes; but traditional physiology never attempted to probe deeply into the nature of voluntary behaviour. It was content to investigate reflex

activity, and concede the prerogative of discussing the characteristics of conscious behaviour to moral philosophy. *Experimental physiology like experimental physics is an ethically neutral science.* If Pavlov has reduced the problem of conscious behaviour to the same level of discussion as the problems of reflex behaviour, the traditional distinction between reflex and voluntary activity has ceased to define the boundary at which physiology ends and moral philosophy begins. If the investigation of the characteristics of conscious behaviour can be brought within the scope of an ethically neutral method, we must abandon any hope that biology can assist to end " the divorce of science from the affirmations of æsthetic and ethical experiences ". If the Relativists can say that modern physics has given materialism its death blow by referring solid matter to an atomic nexus of conceptual fields of force which only exist in our consciousness, the physiologist can add that modern biological enquiry is disintegrating consciousness into an atomic nexus of reflex arcs. If modern physics has shown that we can no longer think profitably of solid matter as existing in the way in which it presents itself to common sense, modern biology is showing that for the purpose of profitable discourse mind itself does not exist as the essential unity which it assumes to common sense. If the advance of science has disposed of the older forms of materialism, it is also disposing of the traditional forms of idealism and dualism at the same time.

Biological science no less than physics is opening up new fields for exploration in philosophy. The new approach to the problem of " conscious behaviour " involves an intellectual effort no less repugnant than the non-Euclidean space of the relativist. The new, in preference to the tradi-

tional biological standpoint, owes its sanction to the same test as that by which relativistic theories must in the last resort be assayed. A disinclination to discuss " conscious behaviour " and a tendency to assert dogmatically the possibility of reducing it to purely chemical concepts has been characteristic of the mechanistic standpoint in the past. It would seem that to emphasize the applicability of physical methods to living matter in all its aspects, a new term, free from this taint, is now needed. *Behaviourist* has already acquired certain restricted implications, and for reasons which will be set forth in a succeeding essay I shall sometimes speak of the *publicist* in preference to the mechanistic standpoint. Unlike the Flat Earth doctrine of Mr Breach the publicist standpoint in philosophy is not based on an appeal to common sense.

I began these introductory remarks by calling attention to two characteristic features of contemporary thought, the uneasy recognition of the opposition of science to common sense and the renewed search for a working agreement between the claims of science and of moral philosophy. In concluding, I would add a third to which I have not explicitly referred. The scientist brought into collision with common sense has for the time being lost his former air of self-confidence. We are told that science does not deal with reality, that the external world of physics is a shadow world, that the laws of physics are only statistical generalizations, that scientific hypotheses are no more than convenient devices to aid us in the practical business of living. I cannot but feel that the solemnity of these assertions is out of all proportion to their novelty. I cannot discover why recent developments in physics constitute a specially cogent reason for reiterating them at the present moment.

I am disposed to believe that some of the younger generation who have been familiar with the writings of Mach, of Pearson, of William James and of Bergson, since they first began to think about the nature of scientific knowledge, must share with me a sentiment of surprise, when told that living scientists ever seriously put forward those extravagant claims which, as we are now assured, received their death blow from the theory of a relativity and the new quantum mechanics. The apologetic attitude so prevalent in science to-day is not a logical outcome of the introduction of new concepts. It is based upon the hope of reinstating traditional beliefs with which science was at one time in open conflict. This hope is not a by-product of scientific discovery. It has its roots in the social temper of the period. For half a decade the nations of Europe abandoned the exercise of reason in their relations with one another. Intellectual detachment was disloyalty. Criticism of traditional belief was treason. Philosophers and men of science bowed to the inexorable decree of herd suggestion. Compromise to traditional belief became the hall-mark of good citizenship. Contemporary philosophy has yet to find a way out of the intellectual discouragement which is the heritage of a World War.

The physicist has abandoned teleology in his own field. He has banished the spiritual values from the domain of his enquiries. He now looks to the biologist to shoulder the task of proving that the universe is consonant with our notions of ethical propriety. I shall endeavour to show that the progress of modern biology gives no justification for the belief that such a compromise is possible. In approaching this task my aim is not primarily to advocate the mechanistic conception of life or to criticise the

vitalistic standpoint. Controversy between writers of the mechanistic and vitalistic schools has too often focused attention on whether a complete solution of the Nature of Life can be found within the mechanistic framework. The more significant question is whether any solution can be obtained outside the mechanistic framework. It may be interesting to know how far the biologist has progressed in his enquiry into the Nature of Life. Philosophically it is more significant to understand what methods of investigation have permitted him to advance towards an admittedly partial solution of his problem. To show that the mechanistic conception of life is inadequate is one thing. To show that any alternative and more comprehensive view can be gained by pursuing methods other than the traditional methods of experimental biology is a more difficult task. It appears to me that the mechanist can admit every criticism which the vitalist brings to bear upon his case without weakening its essential strength. If we commence our enquiries with the assumption that it is possible to know everything, we shall be disappointed to find that the mechanistic conception of life does not—and probably never will—find an answer to every question which we may be tempted to propound. In that disappointment lies the false security of the vitalistic standpoint. It was the peculiar merit of Hume's philosophy that he rejected the necessity of making this assumption.

In assessing the respective contributions of the biological and physical sciences to the construction of a Public World, we are investigating the existence of certain characteristics common to all branches of natural science. The method of science is not static. It is ever growing and expanding, incorporating new territories within its empire. For this

reason formal definitions unfortified by an examination of the historic past tend to be superficial and barren. The origins of even the most exact sciences are deeply rooted in the soil of magic. At any stage in the progress of human knowledge particular features may be evident in more than one branch of enquiry. As time goes on fresh similarities present themselves. The exact line of demarcation between the already scientific and what is not as yet scientific is therefore somewhat arbitrary. No definition of scientific method is adequate unless it implies the recognition of a developmental sequence in which new characteristics emerge successively into prominence.

In the natural sciences, as customarily defined, it is essential that the data shall be publicly accredited by the testimony of independent observers. The observation and recording of publicly accredited data is not in itself regarded as an adequate criterion of scientific study, unless the data are arranged or classified in a particular way. Such classification makes it possible to draw inferences which extend beyond the range of the original data. The validity of the relations implied in a particular classification is referred to their capacity to permit us to predict verifiable conclusions. In practice it is necessary to classify the data of a problem in a variety of ways before it is possible to arrive at the type of classification which yields relations satisfying this criterion of validity. This circumstance assists us to draw a rough distinction between a type of enquiry which is maturely scientific and one which is in process of becoming scientific. In the older and more firmly established branches of science, it is evident that severe economy in the initial assumptions promotes the construction of hypotheses which are valid in the sense

defined. Ethical values have been eliminated altogether. The same characteristics are increasingly recognized in newer departments of scientific investigation.

When every criticism of the limitations of scientific method has been accepted, the belief that philosophy can provide a means of solving the problems which lie outside the realm of scientific enquiry still remains to be proved. In the essays which form the third series of this volume, I shall endeavour to discuss whether an enquiry into the nature of reality has any intelligible meaning. With Hume I doubt whether it is possible to attach any significance to deciding whether scientific beliefs are a faithful representation of "reality." Scientific beliefs are specially characterized by their communicability, or, to use the term which I shall employ more frequently, their *publicity*. The fundamental problem of a philosophy which does not presuppose what it sets out to establish is to find what characteristics of beliefs make them communicable. It is by examining these characteristics that we can hope to decide whether the discussion of our ethical and æsthetic predilections can yield conclusions which have the same kind of communicability as scientific beliefs, and, if that is possible, in what manner such discussion must be conducted. I shall endeavour to show that there is a confusion of meaning involved in discussing whether the experiences with which science deals are more or less real than the experiences which moral philosophy has claimed for its parish. The more modest task of deciding whether the conclusions of science have more or less communicability than ethical and æsthetic beliefs is not a problem which necessarily eludes unprejudiced investigation.

I have indicated that an enquiry into the nature of life

and the nature of consciousness presupposes the necessity of formulating the problem in the right way. This task is a necessary preliminary to the analogous question, what is philosophy? The firefly emits light. When we say that we understand what animal light is, we mean that we understand what processes are involved in the production of animal light. The method of science can lay bare the structures in which luminescent materials are secreted and the physical transformation of their chemical energy into visible radiation. Animal light is an unusual characteristic of a species of insect known as the firefly. The light of reason is a peculiar characteristic of certain human beings known as philosophers. We can only say that we truly understand philosophy or the light of reason, if we understand the processes which confer upon philosophers their unusual characteristics. A philosopher brought into being by the process of natural generation develops in an environment which includes inanimate objects and other human beings. He reacts to his physical environment by growth and to his social environment by learning. If the method of science can assist us to elucidate the processes of growth, learning and natural generation, science can assist us to understand what philosophy is. The anatomy of philosophy and the physiology of philosophers are inseparable. We need not be discouraged in pursuing this line of enquiry, because the answer which science can give us at present is incomplete. It is the chief glory of science that its answers are always incomplete. The pitiful failure of introspective philosophy resides in the finality of its answers. Perhaps the most permanent influence of Relativity in the history of philosophy will prove to be the challenge it issued to the finality with which Kant enunciated the concepts of space and time.

I

THE MECHANIZATION OF CONSCIOUSNESS

"Now, in conclusion, the Method which teaches adherence to the true order and an exact enumeration of the conditions of the thing sought includes all that gives certitude to the rules of arithmetic."—Descartes, *Discourse on Method*

The onus of proving that all the properties of living matter can be reduced eventually to problems in physical chemistry or, on the other hand, of denying that such will ever be accomplished, may be laid on the shoulders of those who commit themselves to rash affirmations and denials. If this were the only matter to decide, a discussion of the merits of the mechanistic conception of life could reveal nothing more than a temperamental difference between the disputants. A temperamental difference does exist. The mechanist has a cheerful attitude to knowledge and refuses to capitulate to the fear of the Unknown : the vitalist, a sadder but not necessarily wiser type, finds balm in the limitations and failures of human effort. The average biologist, who has little sympathy either for the heroic or the desperate point of view, maintains a detached scepticism.

Such scepticism has much to commend it ; but scepticism no less than piety can be employed as an excuse for mere intellectual laziness. Between those who advocate the mechanistic conception of life and those who reject it, there is a divergence of outlook more fundamental than usually appears in the course of controversy. Whether the

same set of hypotheses will ultimately serve to interpret the properties of living and non-living matter may be left to the arbitrament of time. For practical purposes a decision one way or the other makes very little difference to the course of biological enquiry. The fundamental unity of scientific method in chemistry and physics is not invalidated by the fact that some phenomena can only be dealt with successfully in thermodynamical terms, while yet others yield only to treatment with the aid of kinetic and molecular hypotheses. It is less important to know how far the properties of living matter can be reduced to physical chemistry than to decide whether the logical structure of biological enquiry is essentially similar to or different from that of physical science. This is an issue of the most far-reaching consequences, not merely for philosophy but for biology as well. Though rarely stated explicitly, it represents the basic divergence of standpoint between the mechanist and the vitalist or holist. It is not merely a matter of taste or temperament : it is profoundly relevant to the way in which biological enquiry continues to develop. In this matter scepticism can only be justified by disinclination to face uncomfortable conclusions.

If the logical structure of biological enquiry is essentially similar to that of physical science, we must entertain the possibility of interpreting the whole domain of living matter without departing from the principle of ethical neutrality. This is not a pleasant possibility to admit ; and it is hardly surprising that few biologists are enthusiastic in committing themselves with regard to it. If we find that there is no fundamental difference between the logical structure of biological and physical science, we cannot follow Dr Whitehead in reviving the hope that scientific enquiry will event-

ually yield conclusions about the universe in conformity with our ethical predilections. If, without modifying the structure of its logic, biological science is capable of annexing as its parish the entire survey of living matter, there remains no nicely defined boundary at which science ends and philosophy begins. Philosophical enquiry must then abandon its pretensions to arrive at conclusions about the universe unaided by scientific discovery. It must restrict its operations to an examination of the logical structure of beliefs. It is therefore remarkable that the biological standpoint has been so little explored in contemporary criticism of traditional philosophy.

During the past two decades there have been three outstanding developments in biological research, the work of A. V. Hill and Meyerhof on the chemical mechanics of muscle, the extension of Mendel's hypothesis by Morgan and his colleagues at Columbia, and the study of the conditioned reflex by Pavlov's school. Of these the first alone represents an advance in the actual reduction of vital processes to physical chemistry. Yet no aspect of biology could be selected more appropriately than Morgan's hypothesis to illustrate its logical unity with the study of chemistry. The study of the conditioned reflex has not as yet enlisted the resources of physical chemistry to any noticeable extent. Nor does it employ a logical technique as elaborate as that of the modern chromosome hypothesis. Its importance lies in the fact that it has emancipated biological study from the Cartesian dualism with its implicit assumption that method of enquiry applicable to one aspect of the properties of living matter is of a totally different kind from that employed in dealing with the remainder.

To estimate the significance of this advance it is necessary to start with a clear statement about the meaning of a word. The term *reflex* is used by dentists, politicians and faith healers with a variety of implications irrelevant to the biologist. To exclude these irrelevant associations it is best to be concrete. Suppose that we decapitate or destroy the brain of a frog, and suspend it, legs downwards, in a vertical position. On raising a vessel of warm—about 40° C.—water, until the tips of the toes touch the surface, the legs of the animal are withdrawn after a short interval. This event takes place regularly and similarly under the same conditions. It is as definite and predictable a property of secular objects as is the precipitation of barium sulphate on mixing a solution of barium chloride with a solution of sodium sulphate. It is, if you care to express it in that way, a physical reaction between warm water and frog toes. In biological nomenclature it is a reflex.

The word reflex is not used in biology to denote every change that occurs in living matter. To clarify its meaning further we must consider how such a phenomenon can be studied more intimately. To the biologist it presents two types of problem. One is that of analysing the constituent parts of the reaction, and is analogous to what the chemist does, when he determines the solubility and dissociation constants of barium sulphate, barium chloride, sodium sulphate and sodium chloride to define more precisely what occurs during the reaction with a view to elucidating conditions under which it may be expected to occur. In the biological example that we have taken the first stage involves the purely spatial (or anatomical) examination of the reaction. It may be noted in this connexion that anatomy in its initial phase was an experimental science,

and only became a catalogue in its dotage. We observe that we are dealing with a localized response to a localized agent involving a spatially localized structure the nervous system. We can in fact obtain the reaction from a preparation from which every structure but the skin of the toe, the nervous system and the muscles of the leg have been removed. From this point we proceed by a study of the temporal relations of the phenomenon, first undertaken by Helmholtz, to show that a disturbance is propagated at a measurable, predictable and modifiable rate from the seat of application of the agent to the seat of the visible reaction. The further analysis of the problem from the physico-chemical standpoint, an essentially modern development, will be referred to in a subsequent essay. We have now obtained the current definition of a reflex as a localized response to a localized stimulus, involving the intervention of the propagated disturbance known as the *nervous impulse*. Erroneous ideas implied in the common use of the term reflex arise chiefly in connexion with the second aspect of the study of reflex phenomena. This is not readily comparable with the investigation of a simple reaction like the precipitation of barium chloride. It might be compared with the interpretation of a more complex system such as the oxidation of oxalic acid in the presence of potassium permanganate and sulphuric acid, when the behaviour of any two reactants towards one another is already known. Frogs lift their legs from time to time in civil life, when they enjoy the use of a head. We may therefore ask what part do such reflexes, as we can study in the headless frog, play in the behaviour of the intact animal.

In any reflex displayed by the pithed frog the nervous impulse traverses a characteristic path. From the skin,

the receptive area affected, it passes by one of numerous fibres of microscopic thickness to the spinal cord. Such fibres together with others carrying impulses from the cord to the muscles or glands collectively constitute the visible nerves. Fibres carrying impulses into the cord divide into very fine branches in the inner core or grey matter. These fine branches are intertwined with the ramifications of other fibres passing up and down the length of the cord. The latter branch at their other extremities around the fine endings of fibres which pass from the cord to the glands and muscles. An impulse entering the spinal cord first therefore passes across the junction or *synapse* between the fibre along which it enters the cord and some other fibre running up or down the cord. Having traversed the latter, it passes across the junction or synapse between its branched ending and that of some fibre connecting the spinal cord with a muscle or gland. Reflex action depends upon the fact that an impulse travelling along a particular fibre can traverse some synapses more readily than others. This is a physical process, occupying a measurable time. By the use of certain physical reagents it is possible to increase the conductivity of the synapses, so that an impulse entering the cord irradiates to all the muscles of the body. Strychnine is such a reagent.

The familiar fact that the moth flies towards the candle will serve to illustrate how the study of a simple reflex, like the withdrawal of the toes of the pithed frog from warm water, makes it possible to make predictable conclusions about the normal behaviour of animals. If the nerves of the frog's leg are severed, the leg hangs limply. Normally the muscles of the leg are never completely relaxed. They are maintained in a state of partial con-

traction or *tone*, reflexly determined by a number of agencies which for our present purpose it is unnecessary to specify. The nerve fibres which run up and down the length of the spinal cord in the frog cross from one side to the other at some level, and on this account most reflexes obtained in the pithed frog, when only one side is stimulated, involve muscular response of both sides of the body. Insects which move towards the light become noticeably more limp in darkness. Light reflexly increases the tone of their muscles. In insects there is little crossing of fibres from one side of the central nervous system to the other. It follows that, if light reflexly increases tone, the muscles of that side will be more contracted, when one eye is illuminated more strongly than its fellow. This will have the effect of bending the body round in the direction of the incident beam, until the head is brought into such a position that both sides are equally illuminated. Having attained this position the body will continue to move along the direction of the incident beam. If it swerves to the right or left, it is automatically readjusted.

This interpretation of the proverbial flight of the moth towards the candle permits us to make a very large number of easily verifiable predictions. One simple consequence repeatedly confirmed by experiment on a variety of insects which fly towards the light is the fact that, when one eye is blinded, they fly in circles. There is no need to mention the variety of predictable positions which such insects occupy, when allowed to crawl up rotating cylinders illuminated in various ways. One other rather interesting result of the experimental analysis of this phenomenon is worth mentioning. According to the common sense view the insect moves towards the candle, because it likes

the light. There is one and only one fairly evident inference from the teleological way of looking at the matter. It implies that the moth should always fly from the darker to the brighter situation. Now the interpretation of its movement in terms of reflex action signifies that it is the direction of the light rays and not primarily the intensity of illumination which determines the direction of its movement. In Nature moving along the direction of the rays towards the source of light usually involves progression from a darker to a brighter region. In the laboratory it is easy to arrange conditions so that an insect crawling along the direction of an obliquely incident beam, moves from a brighter to a darker area, as it approaches the source. In doing so it behaves, as it would be predicted to behave in such a situation on the assumption that its behaviour is determined by reflex action. According to the teleological view it should do the opposite.

Even in the behaviour of so capricious an animal as man himself, it is possible to isolate units of behaviour to which the term *reflex* is appropriate. The entire behaviour of a pithed frog or of a dog deprived of its brain can be regarded as the summation of a number of discrete reflexes compounded according to ascertainable laws. The problem is not a simple one; but the way in which the operation of one reflex affects the exercise of another has been elucidated with considerable success by Sherrington and his co-workers. Sherrington has paid special attention to what occurs in the simultaneous application of two stimuli whose appropriate responses involve the propagation of impulses along common fibres within the central nervous system. A further complication is introduced by the existence of inhibitory reflexes, responses which involve

the cessation or the diminution of activity already in progress before the application of the stimulus. The work of Magnus and his colleagues, who have solved the riddle of how a cat falls on all fours, demonstrates to a very large extent the possibility of interpreting balancing movements of the body as the summation of such reflexes as are readily exhibited in the brainless or "spinal" animal. Yet few physiologists have ventured to entertain the likelihood that the entire behaviour of even such an animal as a cat, still less man himself, could be treated successfully in this way. Hence has arisen the traditional distinction between reflex and voluntary activity. So long as that distinction was a valid one, biology admitted a fundamental dualism in its subject matter and in its method. The vitalist was in a position to claim that there is a group of properties of living matter in dealing with which we must adopt introspective rather than physical methods of enquiry. The mechanist might reply epigrammatically that physiology deals with what we know about the central nervous system, psychology with what we do not know. The distinction still remained.

There are certain fairly evident reasons why the behaviour of a frog deprived of its brain should be simpler than that of the intact animal. One is that the number of possible paths along which nervous impulses can pass is much smaller. Another is the fact that the brain receives the nerves which bring in impulses from the three great receptor organs, or, in the older terminology, sense organs of the head. The eye and the ear bring the organism within the range of physical influence of innumerable events remotely situated in space. When we have allowed for all such differences there remains a perfectly tangible distinc-

tion between the behaviour of the spinal and that of the intact animal. The response that we have hitherto called a reflex is such that for a given agency under the same external conditions we may expect the same result. There are the best of reasons, based not on any introspective ideas but upon the study of behaviour to make us think that however much we standardize the external conditions at the moment, when the stimulus is applied, we can never predict from that alone exactly what will happen as the result of the application of certain types of stimuli. The performance of "learning" justifies this conclusion, and it has been customary in the past to refer this property of living matter to essentially non-physical concepts such as *memory*. By defining in this way the distinction between reflex behaviour in the traditional sense and voluntary or conscious behaviour, a new problem has emerged. This may be stated in the following way. If instead of concentrating exclusively on what is happening at the moment, we take into consideration the way in which a given stimulus has been presented to an organism on previous occasions, is it possible to establish any relation between the effect it now produces and the events associated with its application antecedently? In so stating the issue we have introduced no new and introspective concepts foreign to the traditional physiology of the reflex. We have simply envisaged the possibility of studying conditions under which new reflex systems may be brought into being.

§ 2

It is this problem which the Russian physiologist Pavlov and his co-workers have attacked with such conspicuous

success during the past two decades. For some time their researches remained little known in this country, but two translations of Pavlov's lectures are now accessible to the English-speaking reader. There is therefore no need to go into details concerning the experimental technique which is formidable. The more significant developments of the subject may be dealt with by considering how aspects of behaviour which were formerly referred to the introspective concepts of memory, attention and sensation can now be investigated without departing from the language adopted by physiologists, when describing the properties of simple reflex action.

Pavlov's investigations commenced with the study of salivary secretion in dogs. A dog which has been deprived of the forebrain secretes saliva, when food is introduced into the mouth. The intact animal also secretes saliva, when food is brought within the range of its eyes or nostrils. In the adult the sight or smell of food is an appropriate stimulus for reflex salivary secretion. The ringing of a bell is ordinarily without effect on the secretion of saliva; but the ringing of a bell if repeated a certain number of times, when food is also presented, eventually comes to evoke salivary secretion, when food does not accompany it. In general it is found that, in the intact animal, a previously indifferent stimulus applied at suitable intervals simultaneously with the application of a stimulus which unconditionally evokes a reflex response is found to acquire the property of evoking the same reflex response, when unaccompanied by the original or " unconditioned " stimulus. A new reflex has been built up. Such reflexes are called by Pavlov *conditioned* reflexes, and the previously indifferent stimulus is called the conditioned stimulus.

Any event in the external world which affects a receptor organ may in the intact animal become a conditioned stimulus, provided external conditions are rigidly standardized in other respects, provided also that it accompanies the unconditioned stimulus a sufficient number of times depending on whether the application is precisely simultaneous, whether the conditioned stimulus begins to operate before the unconditioned, overlapping it in duration or separated from it by a short interval. The task of defining the facility with which a conditioned reflex is built up involves a study of the significance of the interval between successive applications of both stimuli and of the juxtaposition of conditioned and unconditioned stimulus. In defining the conditions which determine the bringing into being of a new reflex system by this method, we are investigating a class of phenomena which would formerly have been attributed to " memory ". At no point is it necessary to depart from the conventions of scientific nomenclature ; and in place of a descriptive epithet, we arrive at a definite specification regarding when and whether an event will occur.

What it has been the custom to denote by the term memory is only one aspect of the problem of " conscious " or " voluntary " behaviour, that is to say those aspects of behaviour which are spatially referable to reflex paths in the fore brain. An animal is constantly subject to the simultaneous application of many indifferent and unconditioned stimuli, but its behaviour is selective. This introduces the problem of *attention*. To ascertain the conditions which prevent new reflex systems from coming into being, or extinguish them when they have become established, was perhaps the most important aspect of Pavlov's work,

because an understanding of this part of the problem underlies the successful control of experimental procedure. The possibility of isolating a conditioned reflex for study implies the existence of some inhibitory agencies which prevent the normal surroundings of the laboratory from exerting a significant influence on the course of the experiment. The inhibition of conditioned reflexes is a complex question; and its complexity emphasizes how broad a basis they offer for the interpretation of "conscious" behaviour in general and the interpretation of *attention* in particular.

From this standpoint two important types of inhibition are called by Pavlov inhibition by extinction and conditional inhibition. The first term refers to the fact that, when an indifferent stimulus has been converted into a conditioned stimulus, and is then allowed to act repeatedly without the unconditioned stimulus, it gradually loses its potency, regaining it after an interval of rest. Conditional inhibition is the extinction which occurs, when a new indifferent stimulus is superimposed upon the effective phase of a conditioned stimulus. A third and especially important form of inhibition is the extinction of a state of inhibition by conditional inhibition, or as Pavlov calls it, inhibition of inhibition. Let us suppose that an organ note of one thousand vibrations per second has been made the signal for salivary secretion by repeated application of the stimulus, when food is administered to the animal. If it is now administered repeatedly without the accompaniment of food, it suffers inhibition by extinction, but recovers its efficacy after a period of rest. If, during the indifferent period, the experimenter superimposes on the now ineffective sound stimulus another indifferent agent

such as the flash of a lamp before the dog's eyes, secretion of saliva ensues. The sound regains its efficacy as a conditioned stimulus. One other type of inhibition which can be studied experimentally is " generalized inhibition " or elimination of the activity of the fore brain, which can be brought about in the dog by local warming or cooling of an area of the skin. This has an intimate bearing on the phenomena of sleep and hypnotic trance, as also on the advantages of summer time.

Perhaps the most radical consequence of the line of work which we are now considering lies in the possibilities which it presents for inverting our traditional attitude to the discussion of " sensation ". When we can isolate some simple unconditioned response to a particular stimulus, we can investigate the extent to which the efficacy of the stimulus is localized with reference to some receptive area, and discuss the sense organ in the same way as a piece of physical apparatus. We know for instance that a frog does not respond to white or black background by the appropriate change in colour of the skin, if its eyes are removed. The influence of the earth's gravitational field on the way in which a frog maintains its normal balance in swimming provides another illustration of the way in which the experimental biologist deals with the phenomenon of receptivity, when it is possible to isolate a type of response which invariably accompanies a particular type of stimulation. In this instance the receptor is that part of the internal ear known as the labyrinthine organ. After destruction of the labyrinthine organ on one side only, a frog swims in a spiral path. If the internal ear of both sides is removed, it swims hither and thither, as likely as not upside down or sideways without any sign of its normal main-

tenance of balance. The inner ear of the frog or man with its three semicircular canals in the three Cartesian planes is a rather elaborate example of a type of receptor organ represented in shrimps by two little sacs called statocysts at the base of the feelers. These sacs contain concretions of sand known as the statoliths. Experimentally the sand can be replaced by iron filings. If this is done, the shrimp swims upside down, when a strong electromagnet is placed above it. The position occupied by the statolith in its sac is determined by the pull of gravity in ordinary circumstances. When the body is bent, the statocyst comes into contact with a new portion of the wall of the sac, thus stimulating a different set of nerve fibres, and initiating appropriate muscular reflexes. The balancing movements of a shrimp in swimming also depend on the eyes. With both feelers removed a shrimp swims normally in daylight. It loses its balance completely in a dark room ; and swims on its back if illuminated from below. Removal of one eye or one statocyst does not affect its balance in daylight, unless the two operations are performed on the same animal. It then swims in spirals.

A modern biologist adopts to the statocyst and the eye the same attitude which he would adopt to the self starter of a motor car, if he were quite ignorant of its mechanism. Sometimes his problem is further complicated by the necessity of turning on the switch before the engine will start, adjusting the spark or cutting down the air. In an animal whose behaviour is largely conditioned behaviour, it is not so easy to isolate simple invariable responses to particular types of external agency. We lapse into the language of introspective psychology. Pavlov has shown that this is unnecessary. By employing the method of building up

conditioned reflexes to define the limits of discrimination, the analysis of sensation can be carried out without departing from the attitude which we adopt to a motor car. Let us suppose that the sound of a tuning fork of 256 vibrations per second, i.e. middle C, is accompanied by electrical stimulation of the paw of the dog, until the note itself becomes an effective stimulus for withdrawal of the paw. A tuning fork of 264 vibrations will also evoke the withdrawal of the paw; but the application of the second stimulus suffers inhibition by extinction before the original (middle C), as can be shown by applying the latter after response to the tuning fork of 264 vibrations has been extinguished. Applying series of tuning forks in such experiments it is found that the limits of discrimination in dogs is a fraction of a tone. The delicacy of this method of testing discrimination or selective receptivity to a given range of stimuli depends on the fact that it is possible not merely to show whether one stimulus can be substituted for another in a conditioned reflex but to measure the extent to which a given stimulus can replace another. Judged from this standpoint dogs and cats are colour blind, as far as such a statement can have any tangible meaning. That is to say, differences of light intensity but not of wave length in the effective range determine the reactions of these animals to photic stimuli.

§ 3

In the light of Pavlov's work the problem of conscious behaviour, or as we should now say conditioned behaviour, no longer presents itself to biological enquiry as a domain in which the methods of traditional physiology must be abandoned in favour of introspective speculation. It

becomes the problem of defining how new reflex systems can be built up. The possibility of a further analysis of the process on mechanistic lines will be discussed elsewhere. Whatever success attends such an attempt, the fact remains that the controversy between the mechanistic and vitalistic schools must now be conducted on a new basis. Mechanistic biology could not claim to take a comprehensive view of the properties of living matter, so long as it failed to indicate how " voluntary " activity, as it was almost universally denoted by physiologists, differs from reflex activity. It is true that some of the more radical mechanists like Loeb preferred to speak of associative behaviour as having a more objective flavour. But Loeb's own use of the concept of " brain images " emphasizes how fundamental is the innovation which the work of Pavlov's school has introduced into philosophical discussion. The mechanist never legitimately claimed more than the right to investigate the properties of living matter in its simpler manifestations by those methods whose success had been justified in the domain of physics and chemistry. If the mechanist ventured to speculate beyond those limits he transgressed his terms of reference. Until the publication of the work of Pavlov's school physiology was tied hand and foot to the traditional distinction between reflex and voluntary behaviour. Thus the author of a standard work on human physiology with a distinctly mechanistic tendency writes on the functions of the cerebellum: " . . . the degree of consciousness, if any, exhibited by the cerebellum is of a much lower order than that shown by the cerebrum. All observers agree that there is no apparent loss of sensation after removal of the cerebellum, but Luciani, Russell and others state their belief that in some indefinable way it is

affected by such operations. Whatever functions of this kind are present we can define only by the unsatisfactory terms of subconscious rather than unconscious . . ." What Howell wrote in 1905 might have been written by any mechanist of that period. The physiologist inevitably lapsed into introspective terminology, when dealing with brain physiology; and it is this restricted mechanistic outlook which Dr Haldane has attacked in his recent Gifford Lectures. It is not difficult to show that the mechanist, as that term is used by Dr Haldane, accepted implicitly the Cartesian compromise. It is surprising that, although Pavlov's work has been generally accepted by contemporary biologists, Dr Haldane completely refrains from considering its bearing on the present status of the mechanistic conception of life.

Dr Haldane's statement that the method of traditional, i.e. mechanistic, physiology " tells us nothing, however far we may extend it, regarding the distinctive characters of conscious behaviour " is especially remarkable. Although few writers have hitherto ventured to formulate the far-reaching philosophical consequences of Pavlov's work, more than fifteen years have passed since the veteran physiologist Sir William Bayliss made the following pronouncement :

> " Pavlov states that he was struck by the fact that when the physiologist leaves the study of the simpler parts of the central nervous system which he has investigated by the observation of reflexes, and proceeds to the higher parts, his methods suddenly change. He gives up the observation of the relation between external phenomena and the reaction of the organism to them and introduces psychological ideas, derived from his own internal consciousness. To extend to the higher centres the method of observing what changes in the organism are correlated with external changes might appear too difficult, but Pavlov has succeeded in doing so to a remarkable degree " (*General Principles*, 1914, p. 502).

In denouncing the mechanistic view of life as set forth by Professor Donnan at the meeting of the British Association in 1928, Dr Haldane states :

> " I regard this view as now entirely obsolete, since it ignores the facts, and this is far more evident now than it was a few years ago, before physiology had become to so large an extent a *quantitative science* " (italics inserted) " . . . The fact that Professor Donnan, though his work in physical chemistry commands universal respect among those who know it, is not a physiologist, may partly account for his opinions."

Perhaps also the fact that Dr Haldane, whose work on the physiology of respiration and excretion commands universal respect among those who know it, neglects in his Gifford Lectures to make any reference to the work of Pavlov may partly account for his belief that " a biologist interprets his observations in a different manner from a physicist " (p. 97). It is certainly permissible to state that Dr Haldane is not speaking for biologists as a whole, when he denies that the problem of conscious behaviour can ever be attacked successfully by the traditional method of the physiologist.

Biologists may be expected to differ in the hopes they may entertain as to the progress of further investigation. We can at least envisage the possibility that biology will advance towards a comprehensive account of the properties of living matter without interpreting its observations in a manner different from that adopted in physics. The work of Pavlov's school shows that it is not necessary to introduce concepts foreign to other parts of biology in dealing with conscious behaviour. Of late years the notion of matter which is so fundamental to common sense has been disintegrated by the advance of the physical sciences. The notion of mind or consciousness so fundamental to

common sense is being disintegrated by contemporary biology in an analogous way. If materialism in the traditional sense is dead, idealism in its traditional form is dead. Like traditional dualism they are dead because they never contained within themselves the capacity for growth. The success of biology in attacking the problem of " conscious behaviour " in Haldane's terminology has been consistent with the attitude of treating *conditioned behaviour* as an aspect of the properties of a peculiar kind of matter, living matter. In that sense the new philosophical outlook which emerges from Pavlov's work is a materialistic one.

Physiology has at length discovered a neutral ground for the investigation of the problem of learning. If it is too early to predict the final outcome of this advance, it is permissible to proffer some tentative suggestions concerning its influence on the future of philosophical discussion. From Plato to modern times philosophical enquiry has mainly occupied itself with what Kant calls " the problems of mere pure reason ". Of these Kant enumerates God, Freedom and Immortality as the three principal objects of philosophical enquiry. For the final solution of these problems, Kant asserted that " philosophy stands in need of a science which shall determine the possibility, principles and extent of human knowledge *a priori* ". Introspective psychology was the " science " to which he assigned this task. Introspective psychology has failed to fulfil the expectations which Kant entertained, when he concluded the *Critique* by expressing the hope that it " would bring reason to perfect contentment in regard to that which has always, but without permanent results, occupied her powers and engaged her ardent desire for knowledge ". The type of psychology which Kant promoted

had already begun to sever its connexion with moral philosophy before the emergence of the Behaviourist tendency in an explicit form. Kant did not refute Hume's arguments when he proposed the question, "whence could our experience acquire certainty, if all the rules on which it depends were themselves empirical and fortuitous"? He stated a problem. For its solution he lacked a method. For its discussion he lacked a vocabulary. If the physiology of human learning continues to progress under the Behaviourist influence to which Pavlov's work has given birth, Kant's solution of the problem, which he himself propounded, must eventually be relegated to the same status as astrology and palmistry in the history of human knowledge.

The strength of Kant's case against Hume's empiricism lay in the immature state of physiological knowledge, when the *Critique of Pure Reason* was published. Kant's views on Space and Time were circumscribed by the biological limitations of his period. The Kantian conception of experience was defined by the influence of light, sound, chemical stimuli, mechanical pressure and temperature affecting the eye, the ear, the nose, the mouth and the skin—the only receptor organs recognized by the physiologists of the eighteenth century. Two of the most important instruments of receptivity in the human body, the labyrinthine organ and the proprioceptors which respond to the state of tone of the muscles, were not studied till the nineteenth century. If Kant had been familiar with the physiology of the labyrinthine organ, he would not have argued with the same cogency that the concept of space is essentially different from the concept of weight. The *a priori* necessity of the proposition that "space has

only three dimensions" was determined, according to Kant, by the existence of an "external sense" which is "a property of the mind". If he had lived fifty years later he would have realized that the "necessity" of the Cartesian frame work is a material consequence of the structure of the internal ear. If Kant had been familiar with Sherrington's work on the proprioceptor organs, he would have seen a deeper significance in the experiment which Galileo performed, when he used his own pulse to measure the period of a swinging lamp. Kant was compelled to attribute the "*a priori* necessity" of the proposition that "time has only one dimension" to "the internal sense by which the mind contemplates itself". The time conditioned reflexes which Pavlov has demonstrated are intelligible to modern physiology without recourse to a "faculty of pure *a priori* cognition". The human body is itself a clock from whose tickings we can never escape. Periodic changes in tone of the body muscles influence the proprioceptor organs in a manner essentially analogous to the way in which light exerts its effect on the eye.[1]

Kant's physiology calls for more detailed treatment elsewhere. In concluding this essay, I must remove one source of misunderstanding. I do not assert that all aspects of conscious behaviour will eventually be explained in terms of Pavlov's conditioned reflexes. I do affirm that Pavlov has successfully applied the methods of traditional physiology to the study of processes presumably included in Dr Haldane's definition of conscious behaviour. The strength

[1] That a volley of afferent nerve impulses passes along the vagus nerve to the brain at each heart beat is a fact which can be physically demonstrated with the Einthoven galvanometer.

of Dr Haldane's position lies in the fact that behaviour ceases to be called conscious so soon as it is possible to bring it within the range of scientific prediction. I can well believe that the vitalists of fifty years hence will be assuring their opponents that they never regarded the process of learning, the phenomenon of attention or sensory discrimination as characteristics of the conscious state.

II

THE ATOMISTIC VIEW OF PARENTHOOD

> "When you can measure what you are talking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of science whatever the matter may be . . ."—Lord Kelvin, *Addresses*

The future progress of biological science depends upon a large number of unpredictable contingencies, some political, others meteorological. The collision of the earth with a comet may leave the fate of the argument between the mechanist and the vitalist for ever unsettled. There is therefore no justification for a dogmatic assertion that all the properties of living matter will eventually be reduced to the same hypotheses as are adopted in physical chemistry. But it is doubtful whether any biologists of the mechanistic persuasion have on any occasion explicitly committed themselves to so rash a statement. The vitalistic Sarah Gamp has invented a mechanistic Mrs Harris with the express object of giving her a piece of her mind. As a polemical device this is most valuable, especially in political propaganda. It does not help the mechanist to understand what vitalism can offer as a guide to further biological enquiry. His perplexity is increased by the circumstance that so many vitalists of the platform behave themselves with mechanistic propriety in the laboratory. Dogmatism is at least as frequent among those who call themselves vitalists as among mechanists. The vitalist does not

qualify his denial that a complete solution of the riddle of life can be obtained in physico-chemical terms. The mechanist is usually content to state that he knows of no other terms in which an intelligible solution could be found. The vitalist even goes further, and, quite inconsistently with his laboratory practice, if he is a competent biologist, asserts, that in its very methodology, biology is an *independent* science. A biologist, says Dr Haldane in his Gifford Lectures, " interprets his observations in a different manner from that of the physicist ".

This I think is the main bone of contention between the two attitudes which are generically denoted by the terms mechanistic and vitalistic. The real issue has shifted from deciding whether the hypotheses of physics and chemistry suffice for the interpretation of vital phenomena to deciding whether there is an essential difference between the logical structure of those branches of science that deal with living matter and those which deal with inanimate objects. This is a welcome change, because it presents a much more genuine and concrete problem for solution. It is somewhat surprising that the controversy should undergo such a metamorphosis at the present moment. The recent development of evolutionary biology is especially calculated to reinforce the belief that biological theory only progresses, when the biologist adopts towards the subject matter of his investigations the same attitude as that which the chemist and physicist adopt towards the objects which they study. In our generation it is possible to find in those aspects of biology which are most recalcitrant to the application of physico-chemical hypotheses the most conspicuous examples of a fundamental similarity in the logical procedure which the biologist on the one hand and the physicist or

chemist on the other employ in constructing their hypotheses. It would not be possible to select from the whole field of biological science a more striking illustration of the success of quantitative and experimental methods than the recent extension of Mendel's hypothesis by Morgan's school. This advance has entailed an extensive elimination of teleological concepts in the interpretation of the evolutionary process. Yet the phenomena of heredity and variation at present lie completely outside the scope of physico-chemical analysis in the ordinary sense of the term; and any attempt to formulate the problems of genetics in physico-chemical terms is still a matter of pure conjecture.

In this sense we may agree with one writer of the vitalistic school in saying that to speak of the "mechanism of heredity" is a meaningless collocation of words. But if our interest is primarily directed not to the end product itself but towards the way in which the scientist proceeds to elaborate his hypotheses, the study of heredity provides a particularly clear example of how a hypothesis developed without any departure from the *principle of mechanism* can yield verifiable conclusions about the behaviour of living systems. From this point of view it is both legitimate and intelligible to speak of the mechanisms of heredity and variation; and the expression is as permissible as the analogous phrase, the mechanism of chemical reaction. A comparison of the growth of the Mendelian principle with Dalton's atomic theory of the structure of matter will help us to see whether the biologist does actually interpret his observations in a manner different from that adopted by the student of non-living matter, and whether the biologist has recourse to a kind of logic which is different

from the logic which the physicist and chemist employ in framing their own generalizations.

When Mendel took up the problem of hybridization, the nature of fertilization in plants was known in a general way. Just a century before Mendel began his work Kolreuter by painting pollen from one individual on to the stigmas of another variety, and vice versa, had shown that hybrids inherit equally from the pollen and seed plant. At the end of the eighteenth century and the beginning of the nineteenth, Knight and Goss in England had made further progress in crossing pure bred varieties by calling attention to the " splitting " of hybrids, or reappearance of parental types when intercrossing hybrid offspring. Contemporaneously with Mendel, Naudin in France studied this phenomenon more closely, and came very near to formulating Mendel's principle. His results were published in 1862. These pioneers in hybridization laid down the necessity of working with what to the geneticist is like pure chemicals to the chemist, pure breeding stock. They fell short of arriving at far-reaching results, because their attitude to heredity was dominated by the holistic standpoint. They could only think of the plant in terms of a preconceived notion of individuality. They refrained from focusing their attention on the separate parts, and following out the fate of discrete characteristics in their crosses.

We must not overlook the debt which Mendel owed to the pioneers of hybridization. There would have been no modern chemistry if the Arabs and alchemists had not devoted years of laborious study to the clarification of our idea of a pure substance ; and there would have been no genetics, if the idea of pure breeding stock had not been laid down by Mendel's predecessors. Chemistry failed to

progress beyond the stage of describing new compounds so long as it remained entangled in the vitalistic " phlogiston " concept ; and genetics, the study of heredity and variation, remained purely descriptive, until it was emancipated by Mendel from the holistic tendency to concentrate upon the organism as a whole. Naudin did in fact envisage less definitely than Mendel the atomistic concept of heredity, just as William Higgins had partly visualized the chemical possibilities of atoms before Dalton published his theory.

Mendel used in his researches pure breeding stocks differing only in well-defined particulars, employing single characteristics as units of study, and recording the progeny of every cross separately for comparative observation. In his original work Mendel chiefly dealt with the common pea, which possesses two advantages which recommend it for such experimentation, namely, that its flowers are capable of self fertilization (i.e., the pistil can be pollinated from the stamens of the same flower) and that it has a number of well-marked varieties distinguished by tangible characteristics such as the shape (round or wrinkled) and colour (green or yellow) of the seeds, or the stature (tall or dwarf) of the shoot, etc. In all his crosses involving a single difference of this kind he found that the first generation of the cross resembled one of the parents. When these crossbreeds were self fertilized, they produced offspring resembling the original parents in the constant ratio of three to one. One-quarter of the offspring of the crossbreeds resembled one parent and bred true ; one-quarter resembled the other parent and bred true ; and the remaining half being like the " dominant " parent, which the first generation of hybrids resembled, behaved exactly like the latter, when self fertilized.

An investigator who had not the attitude which makes a capable chemist might have been distracted by the peculiar circumstance of dominance, or the resemblance of the impure individuals to one of the parents exclusively. Mendel rightly judged this to be insignificant. A chemical analogy will perhaps assist to make this clear. Sodium and potassium yield colourless salts with most common acids, but the permanganates of both are purple in solution. The salts of copper are generally of a bluish or greenish tint in solution. In the one case the anion, in the other case the kation, is the dominant factor in determining the physical property of colour ; but in both cases the other component behaves in any reaction with no less characteristic efficacy, because its presence is seemingly masked. So likewise Mendel looked beyond the bodily resemblance of the dominant parental and hybrid individuals to their hereditary makeup ; and recognized in his experimental data two general conclusions which prompted special consideration. One was the fact that the original parental types can be recovered in all their purity. The other was the fact that the various hereditary types produced by hybridization regularly appear in the same numerical ratios. Both conclusions are of universal validity, though Mendel had the very good fortune to select materials which yield the simplest type of numerical results which occur in crosses between pure strains. When Dalton formulated the atomic hypothesis two fundamental empirical generalisations of chemistry were fully accredited. The law of the conservation of matter and the law of constant proportions had been established. Mendel found in his data the proof of what we might call the principle of the conservation of genetic materials and the law of constant genetic pro-

portions. To the recognition of these empirical generalizations he added a conceptualization of the basis of their existence in terms of discrete factors. These factors were according to Mendel's hypothesis (or Mendel's " first law ") units of hereditary combination, just as Dalton's atoms were units of chemical combination.

Each character involved in his crosses was regarded by Mendel as determined by a factor derived from the maternal and one derived from the paternal parent. A pure individual was thus represented by *aa* or *bb*, and an impure individual by *ab*. Mendel assumed that *a* and *b* are atoms of heredity in the sense that they retain their separate entities through the whole course of development. Having introduced this conception, he showed that all his numerical data followed from the laws of chance, if the maternal and paternal factors which determine a particular character separate in the formation of the gametes (pollen and ovules) so that one-half of the gametes contain only the maternal and one-half only the paternal factor for the character considered. The combinations which may occur as the result of fertilization are compatible with the assumption that any given male gamete (pollen or sperm) may fertilize any given female gamete (ovule or egg cell). Mendel's first law may then be stated thus : characters distinguishing different hereditary strains depend upon factors which are inherited from both parents and *segregate* in the formation of the gametes, so that one-half contain the paternal and one-half the maternal factor. Mendel tested the implications of this hypothesis by crossing his hybrids to pure types with verifiable results. He then proceeded to make crosses involving two or three character differences. This led him to enunciate a second law which might be compared with the law of multiple

proportions in chemistry, for its validity is of less general significance than the first law. It served eventually to direct attention to the much more complicated numerical results which arise in dealing with character differences attributable not to one but several pairs of factors. The analysis of such cases was left to Mendel's successors.

There is internal evidence in Mendel's writings to support the view that Mendel himself realized that the atomistic conception of inheritance would demand a drastic revision of the prevailing notion of variation. To Mendel's generation, to Darwin and the pioneers of Natural Selection, variation and heredity were co-extensive terms. Offspring were always on the whole like their parents, but always on the other hand a little different. So the species in conformity with sound liberal principles broadened down from precedent to precedent. But on the atomistic view heredity is essentially conservative, and variation essentially revolutionary. For an indefinite number of generations the atoms of heredity remain unchanged. But times come, when the political barometer falls, and the change when it happens is a discontinuous one. Something new has been brought into being, as when lead is produced from the disintegration of radium or another allotropic modification of an element is formed. The full implications of this were not destined to be realized till forty years had elapsed. Meanwhile the evolutionary ship drifted upon an uncharted ocean of speculation without the compass of experiment to direct its course.

§ 2

Mendel's work published in an obscure horticultural journal remained neglected for forty years, till in 1900

his principle was independently rediscovered by three continental workers—de Vries, Tschermak, and Correns. During that period the study of the reproductive process had progressed rapidly. The way was being paved for new and spectacular developments of the atomistic standpoint in heredity. To appreciate the subsequent elaboration of Mendel's hypothesis in its historical perspective a brief digression into the anatomy of the cell is necessary.

Mendel's researches were confined to plants. When he started his work the nature of fertilization in animals was still obscure. The bodies of animals like plants were known to be built up of microscopic bricks, or *cells* as Robert Hooke had called them. With the use of more powerful microscopes this had gained general recognition during the thirties and forties. Two centuries had elapsed, since Leeuwenhoek with the first microscope had seen seminal fluid teeming with minute vibratile bodies, the *spermatozoa*. At the end of the eighteenth century that inquisitive ecclesiastic Spallanzani had shown that the sperm is the essential constituent of the seminal fluid. By 1841 Kolliker had traced the development of the spermatozoa from single cells of the testis. It was not until 1875–9 that Hertwig and Fol working on sea urchins independently observed for the first time in history the penetration of the egg by the sperm, and established the universal rule that fertilization involves the union of a single sperm with a single egg cell. All modern discussion of genetic differences takes its starting point from the fact that anything which is implied by the word inheritance has its material basis in the microscopic sperm contributed by the father or in the egg cell with which it unites.

In all animals the sperm is a microscopic entity. In all

animals from the jellyfish to Man with very few exceptions its appearance is extraordinarily similar. It consists of a thicker portion to which is attached a long vibratile process, or flagellum. The eggs of different animals are of very different dimensions. Sometimes they contain immense stores of food material (yolk). Sometimes as they pass to the exterior by the female generative tract they are invested with an additional slimy coat and a leathery or calcareous shell, secreted by special glands. The immature egg of all animals is essentially similar. In the living condition it is a spherical or ellipsoidal body in which a clear spherical vesicle is seen ; this vesicle present in all cells is called the *nucleus*. The thicker part or body of the sperm consists mainly of the nucleus of the cell from which it is derived. At fertilization it swells up and unites with the nucleus of the egg. The fertilized egg then divides into two separate segments or cells, and the process of dividing is repeated an indefinite number of times. The cells or segments into which the fertilized egg divides each contain a nucleus, and the process of segmentation which involves the division of cells into two is accompanied by the division of the nucleus of each dividing cell. Like the testis or ovary the substance of all the organs of the animal body is built up of the microscopic bricks which we have called cells. In some tissues like bone and cartilage the bricks are separated by a good deal of mortar. Others, such as the lining membranes of the body, consist simply of cells packed tightly together. At the beginning of embryonic existence all the cells are very much alike. In the course of development the cells of different tissues are considerably differentiated. Throughout all stages the process of cell division always involves the partition of the nucleus in a highly characteristic manner.

The details of this peculiar process, first elucidated by Flemming and others during the seventies, has proved to be of astonishing significance for the further understanding of Mendel's hypothesis. When a cell is about to divide, the nucleus looks like a tangle of fine threads; and this tangle of fine threads resolves itself into a number of readily distinguishable filaments which become progressively shorter, assuming the appearance of stout rods staining deeply with basic dyes. These rods, visible only with high powers of the microscope, are the *chromosomes*, whose behaviour has provided us with a tangible basis for Mendel's conception of inheritance, and have thereby permitted an extensive clarification and amplification of the original hypothesis. From one point of view they might be said to have done as much for the Mendelian conception of heredity as the discovery of alpha particles has done for our belief in the atomic structure of matter. As the dividing cell begins to constrict, the chromosomes arrange themselves at its equator, and split longitudinally into halves, each half travelling to opposite poles, where they spin out again into fine threads from which the nuclei of the daughter cells are built up. Thus each of the chromosomes in the nucleus of any cell in the body is structurally equivalent to a corresponding chromosome in the preceding or succeeding cell generation. About the year 1875 it was recognized that this numerical constancy extends beyond the life of a single individual. In every species of animal or plant the number of chromosomes which can be counted in dividing nuclei is a constant for the species.

With the discovery of this fact a new problem arose so soon as the essential features of fertilization were appreciated. How is this constancy maintained from generation

to generation of new individuals? Two investigators, Van Beneden and Boveri (1881–3), who worked on the horse threadworm, a form which has only four chromosomes in the dividing cells of the segmenting egg, showed that the egg and sperm each contain only half the number of chromosomes characteristic of the cells of the embryo. This conclusion turned out to be a perfectly general one. Attention was immediately directed to the nuclear changes which happen in the formation of the gametes. Innumerable cell divisions occur in the testis or ovary of an animal. These are at first similar in all respects to those which occur in the segmentation of the developing embryo; but cell division goes on in the testis or ovary throughout life. If we trace backwards the history of an individual sperm or egg in the testis or ovary in which it originates, we find a reduction of the number of chromosomes effected during the last division but one, leading up to the formation of a sperm or ripe egg. This penultimate division of the germ nuclei is preceded by the fusion of the chromosomes lengthwise in pairs. When the division actually takes place, each pair behaves like a single chromosome, splitting in such a way that one member of each pair goes to form each daughter nucleus. The succeeding division being normal, each gamete receives half the number of chromosomes present in ordinary cell division. At fertilization the normal number is restored. Thus each ordinary cell of the body has a chromosome set of which half the components are paternal and half maternal in origin.

In many animals and plants the chromosomes are very distinctly of different sizes and shapes, and can be sorted out into corresponding pairs. Such arrangements are constant for the species, and could only be maintained constant,

if each gamete contains one representative of each pair. This means that the maternal and paternal constituents of a pair are distributed in the reduction division to different cells. The chromosomes therefore exist in pairs of which one element is of maternal origin and one of paternal origin. Each gamete receives one element of each pair, just as Mendel supposed that each gamete contained either the paternal or maternal element of his paired "factors". By a curious coincidence this far-reaching conclusion was first established in the very year which witnessed the application of Mendel's principles to animals by Bateson in England and Cuenot in France (1902). Its recognition accompanied the elucidation of another peculiarity of nuclear division, also destined to have important theoretical consequences. In many animals there is found to be an unequally mated pair of chromosomes, the XY pair. When this occurs, it occurs in one sex only. In the alternate sex there is a corresponding equal pair (XX). In birds and moths the female is the XY, the male the XX individual. In other animals the male is usually found with sufficiently careful measurement to have an unequal (XY) pair which is equally mated in the female (XX). During the nineties it was found that some animals had in one sex an odd number of chromosomes, a fact which at first sight seemed to conflict with the numerical constancy of the chromosomes. In the early years of the present century American zoologists provided the key to an understanding of the discrepancy. In all such cases the alternate sex has one more chromosome. The case of the large cockroach will serve as an illustration. The male of *Periplaneta americana* (its technical name) has 33, the female 34 chromosomes. The eggs will all have 17 chromosomes. One-half of the sperm will have 17, the

other half 16 chromosomes. If a sperm of the former class fertilizes an egg, the individual produced will be a female (17 + 17 = 34); and if a sperm of the second type fertilizes an egg, the individual produced will be a male (17 + 16 = 33). In an animal with an unequally mated (XY) pair of chromosomes in the male reduction will result in one-half of the sperm carrying the X and one-half the Y chromosome. The eggs will all have the X, since this chromosome is equally paired in the female. Thus an egg fertilized by a Y-bearing sperm will become a male, while an egg fertilized by an X-bearing sperm will develop into a female.

§ 3

By statistical reasoning Mendel had deduced from his experimental data the existence of entities which behave just as the chromosomes do. He had no direct evidence that his factors had any material basis in the architecture of the germ cells. The new cell anatomy provided independent confirmation of his predictions from an unexpected quarter; but it was not immediately recognized that this was so. Antagonism to the belief that the chromosomes fulfilled the requirements of Mendel's hypothesis is easily explicable. To Mendel's first disciples his second and first laws were equally sacrosanct. Mendel's second law implies that different pairs of hereditary factors behave quite independently of one another. On such an assumption one of two deductions is inevitable. Either the applicability of Mendel's first law is extremely restricted; or the number of factors is too large to permit of their localization in the chromosomes. The sweet-pea, for instance, has only seven pairs of chromosomes. If Mendel's second law were as general as the first, only seven pairs of factors could be

accounted for by the behaviour of the chromosomes. From this dilemma further development of the atomistic view of heredity was rescued, when it was discovered that Mendel's second law is only a particular case of the possibilities inherent in the first.

In 1910 Bateson and Punnet first discovered in the sweet-pea what they then called "coupling and repulsion", or as we now say, *linkage*. Without going into the experimental data, we may define the phenomenon of linkage in the following way. Suppose that these are two varieties A and B which obey Mendel's first law and two other varieties C and D which likewise conform to its requirements, when crossed with one another. Mendel's second law stated that in a cross between AC and BD the second generation will consist of the types AC, AD, BC and BD in numerical proportions agreeable to the assumption that it is equally likely that the factor determining A will be present in the same gamete as the factor determining C or the factor determining D. Bateson and Punnet found that this does not always happen. There is another category of cases in which the factor which determines A sticks more or less completely to the factor for C in preference to the factor for D. The detailed analysis of these cases was at first made difficult by Mendel's literal symbolism, and his way of thinking of factors in *pairs*. But the discovery of linkage at once led Lock to formulate the fruitful suggestion that factors located on the same chromosome pair would satisfy the requirements of linkage, while factors located on different pairs of chromosomes would fit in with Mendel's second law.

From this point onwards the most spectacular development came from the study of inheritance in animals, and

the significance of the chromosomes was immensely reinforced by newly gained knowledge of sex determination. Almost contemporaneously with the discovery of the sex chromosomes or XY mechanism, as we now say, Leonard Doncaster had elucidated in moths the phenomenon of sex-linked inheritance. This was soon found to be of common occurrence in animals. Till this discovery, which was made in 1905, the same results had always been obtained in crosses of pure-bred varieties, whether the male or the female parent displayed one or the other characteristic distinguishing them. Doncaster's work on the currant moth showed that there is a category of cases which at first sight obey Mendel's first law in its simplest form when the cross is made in one way, but yield a different type of result when the cross is carried out reciprocally with respect to the sex of the parents. In such cases one sex is only able to transmit certain characters to its offspring of the opposite sex. It was already known that the XY sex (male in Man and most animals) can only transmit its X chromosome to the XX type. The facts did not dovetail at first sight, because sex-linked inheritance was originally elucidated in birds and moths of which the female is the XY type. There was still an attitude of hesitancy towards accepting Lock's hypothesis, strengthened by the persistence of an incorrect interpretation of the process of reduction which had been made the basis of Weismann's metaphysical speculations concerning "germinal selection".

When in 1914 Doncaster summed up the case for regarding the chromosomes as the material basis of Mendel's first law, a new era had already dawned. Thomas Hunt Morgan, the central figure of a group of ardent investigators at Columbia, had initiated a body of enquiries which within

half a decade eclipsed all other achievements that had succeeded Mendel's pioneer labours. About the time when Bateson first encountered the phenomenon of linked inheritance Morgan began to rear the fruit-fly Drosophila for breeding experiments. Till then genetic experiment had been held in check by the slow rate at which most convenient animals and plants reproduce and the expense entailed in breeding them in sufficiently large numbers to permit statistical inference. The fruit-fly completes its life cycle, if kept in warm laboratory conditions, in a period of ten days. It is prolific. It feeds on rotten banana skins. It therefore costs little to breed. To these immense advantages it adds two others of supreme importance. It has only four pairs of chromosomes readily distinguishable from one another by size and shape ; and it has produced in the laboratory a crop of several hundreds of sports or *mutants*. Each mutant type differs from the wild parent stock in some well-defined characteristic inherited in crosses with the wild type in accordance with Mendel's first law. The mutant characters are extremely varied. One is distinguished from the red-eyed parent by having white eyes, another by having purple eyes, another by having no eyes at all. One is distinguished by having wings that are practically vestiges, another by wings that turn up at the tips, another by wings that are truncated at their extremities. From the wild type which has a greyish body, one mutant is distinguished by a deep black, another by yellow coloration. The mutant characters are thus in general clear-cut differences lending themselves to easy identification. With an animal that breeds so rapidly and prolifically information accumulated with astonishing rapidity. From data based on the study of a large assemblage of mutant characters

there soon emerged the precise requirements of Lock's hypothesis. All the mutant characters of Drosophila fall into four groups. Members of the same group always tend to stick together in hereditary transmission. Members of different groups like Mendel's dihybrids behave independently of one another. Of several hundred mutant characters in Drosophila every one belongs to one of these four linkage groups; and the number of chromosome pairs in Drosophila is four.

This discovery was only the beginning of what might well be called one of the faery tales of modern scientific research. In the way of accepting Lock's hypothesis there were still difficulties. It was in evading the principal difficulty that Morgan's school extended the atomistic concept of heredity much further than his predecessors had done. Till then the main outcome of experiments on breeding had been to show that Mendel's principle was of vastly wider applicability than was at first supposed, and to engender the suspicion that the patient unravelling of difficult and elusive cases would establish its universal validity. As yet the world of Mendel's atoms was without form. Morgan and his colleagues gave it a map. Not content with showing that Mendel's atoms of heredity have their material basis in the chromosomes, nor with actually identifying which chromosome is significantly associated with a particular mutant character, Morgan went further and localized the region of an individual chromosome in which a particular Mendelian factor resides. He thus gave to Mendel's factors spatial co-ordinates in the living cell.

At the outset the study of linkage upon which the chromosome map is based was facilitated in the case of Drosophila, because the varieties dealt with were all known to

be mutants from a fixed wild type. Thus it was possible to break away from Mendel's conception of "pairs" of hybridizing characters. The Mendelian factor was replaced by the mutant *gene*, by saying which is implied that a mutant arises because at some point on a particular chromosome a physical change has taken place. The gene is the Mendelian factor for the mutant condition, but no assumptions are made about what determines the wild-type condition. The inter-relationship of different characters is greatly simplified by thinking only of the relation of one *mutant* gene to another. The discovery that all the genes fall into four groups corresponding to the four groups of chromosomes presented one stumbling-block. Members of the same group in general do not invariably stick together. When two mutants are crossed the numerical proportions of the various types of offspring give a definite value for the probability that the gene A and the gene B will stick together or separate apart. This is a constant for A and B. The constant used in practice is the tendency for A and B to separate. Expressed as a percentage, it is called the *cross-over value*. Some additional information was necessary to explain why A and B do not always stick together, if they are associated with the same chromosome. It was from the solution of this problem that the chromosome map took shape.

Here the sex chromosomes came to the rescue. One very interesting type of sport which has turned up in breeding the fruit-fly is not recognizable by any discrete bodily peculiarity but merely by an abnormality in the number of chromosomes. Of these the first to be discovered was a type of female which has in addition to its usual four pairs of chromosomes an additional Y chromosome. The XXY

females yield very extraordinary numerical results both as regards the sex ratio and other characteristics, when used in making crosses involving mutant characters. There is a class of mutant characters in Drosophila, more than a hundred in all, distinguished by the fact that they are not inherited symmetrically with regard to sex. They display linkage *inter se.* They behave as " sex-linked " characters. The introduction of XXY females into crosses involving such mutant characters results in numerical ratios which are inexplicable on any assumption other than the view that the sex-linked gene is referable to the X chromosome alone. Yet, although the sex-linked genes are all borne on the same chromosome, they do not invariably stick together in crossing. The holistic chromosome clearly would not do. An atomistic chromosome had to be put in its place.

The clue to this was provided by studying more closely the extent to which the different genes stick together. Taking all the genes located on the X chromosome this remarkable generalization emerged from Morgan's researches. If A, B, and C are three sex-linked genes ; if the probability that A and B will not stick together is x, and the probability that B and C will not stick together is y, the probability that A and C will not stick together is either the sum or the difference of x and y. The correspondence here stated is, of course, subject to the margin of error permitted by the theory of probability. To interpret this new *law of the linear alignment of the genes* Morgan made use of a structural peculiarity of the reduction process. When the chromosomes pair in the reduction division, they appear to become twisted. The appearance suggests that in the ensuing split corresponding lengths of the original pair are

interchanged. It is very natural to assume that the likelihood that two points will be separated from one another in such a manner is proportional to their distance apart. So if the sex-linked genes are arranged in a series along the length of the chromosome, the probability that A and C will not stick together must be the sum of the probabilities that A and B and B and C will be separated. This is just what experiment had shown to be true. Thus all the genes on the X chromosome can be arranged in a linear series. The intervals between consecutive genes in such a series represents a space dimension.

The law of the linear alignment of the genes was soon found to apply to the other groups of linked characters. Abnormalities in the number of chromosomes have made it possible to identify each of the remaining three linkage groups of the fruit-fly with its corresponding pair of chromosomes. The first chromosome map of Drosophila was constructed in 1916. It revealed the suggestive coincidence that the number of ascertained points on each pair of chromosomes is roughly proportional to its size. There is now very little doubt that the work of the Columbia school has revealed an aspect of inheritance which is of general significance. After years of patient work with the relatively slow breeding sweet-pea, Punnet has at length elucidated seven linkage groups corresponding to its seven pairs of chromosomes. He has constructed a chromosome map of a seed plant on the basis of the principle first established for the fruit-fly. A law which holds good for two organisms so far apart in the evolutionary scale can hardly be supposed to be lacking in universal validity. The chromosome hypothesis may now take its place as one of the major generalizations of biological science. The law of linear alignment

has transformed Mendel's original conception of inheritance in a way which might be compared with the elaboration of Dalton's hypothesis after the discovery of the law of combination of gases by volume. Mendel's atoms of heredity are now units spatially localized in larger units of microscopically visible dimensions. These supermolecules are the chromosomes.

Being a portion of living matter the chromosome is constantly undergoing chemical change. Some critics of the chromosome hypothesis have based objections upon this circumstance. The difficulty is more apparent than real. Like the individuality of the modern atom the individuality of the chromosome must be conceived in statistical terms. For the discussion of the more familiar chemical reactions the statical atom of traditional chemistry is adequate. For the interpretation of hybridization experiments the diagrammatic chromosome of the text-book suffices. In the field of radioactivity the statical atom makes way for a dynamical model. So also in the domain of cell physiology we conceive the chromosome as an ever-changing entity. The logical situation is analogous in the two cases. Those who hold with Dr Haldane that the biologist must interpret his data in a manner different from that in which the chemist or physicist interpret theirs have now to fall back on the contention that the Mendelian view is only a partial picture of heredity transmission. Anything which might have been said in favour of this contention ten years ago has been weakened by recent work on the inheritance of size. The pioneers of Mendelism selected clear-cut hereditary differences which ordinarily manifest themselves in any environment in which the animal or plant can live. They succeeded in showing that a vast number of hereditary

differences involving a great variety of anatomical and physiological features conform to the requirements of Mendel's hypothesis. There was one category of phenomena which remained obscure till quite recently. Differences in size, height, body weight and the like vary greatly with environmental conditions. Two stocks may be distinguished from one another by the fact that the average member of one is measurably different from the average member of another; but any given individual of one stock may be indistinguishable from another individual of the other, because, even when the environment is standardized as much as is practicable, the range of variability of the two stocks overlaps. The analysis of such cases cannot be undertaken by the ordinary technique of Mendelian experiments; but certain statistical requirements of Mendel's laws may nevertheless be verified. By elementary statistical reasoning we can deduce that the coefficient of variability of the progeny of a cross between two inbred stocks must be a minimum in the first generation and a maximum in the second. This has been shown to be true in a number of crosses in which it is impossible to distinguish individual genetic types by direct observation.

There is no longer any adequate reason to support the contention that Mendel's atomistic concept leads us to an incomplete understanding of biparental inheritance in animals and plants. Those who assert that it is so are now forced to fall back upon the last resort of obscurantism by appealing to the magnitude of our ignorance. The modern theory of the gene is a statistical construction consistently developed by a logical interpretation similar to that adopted in elaborating the great generalizations of physical science. The mechanist is often accused of attributing vital processes

to " chance " combinations of phenomena. If the word chance is used to imply that we do not know the precise conditions which determine such combinations, the statement is hardly exceptionable. It might also imply that the phenomena which biologists study can be successfully interpreted in terms of the mathematical laws of chance. The history of Mendelism shows that these laws provide a fruitful basis for predicting the behaviour of living systems, even where physico-chemical hypotheses at present fail to throw light on the phenomena which the biologist studies. The biologist is able to progress to greater certainty of prediction only when he interprets his data with the same logical method employed by the chemist and physicist to deduce physical " laws ". Whatever the future holds in store for further interpretation of heredity and variation on physico-chemical lines, the progress already achieved has at every stage involved elimination of holistic concepts by the ruthless application of mechanistic logic. To the application of physico-chemical hypotheses no branch of physiology has proved more recalcitrant than the physiology of inheritance. No branch of physiology might more suitably be chosen to cast doubt on Dr Haldane's recent statement that " anything which can properly be called scientific physiology is impossible apart from the assumption of *holism*."

III

THE NATURE OF LIFE

"I am sorry then I have pretended to be a philosopher: for I find your questions very perplexing; and am in danger, if my answer be too rigid and severe, of passing for a pedant and scholastic: if it be too easy and free, of being taken for a preacher of vice and immorality. However, to satisfy you, I shall deliver my opinion upon the matter, and shall only desire you to esteem it of as little consequence as I do myself. By that means you will neither think it worthy of your ridicule nor your anger."—David Hume, *The Sceptic*

§ 1

Since a man must needs live before he can be a philosopher, no problem of philosophy is more fundamental than the nature of life. There is also no issue which provides more scope for vague, barren and undisciplined discussion. A Regius Professor of Moral Philosophy, whether he accepts the fact with resignation or refuses to do so, is a piece of living matter. Perhaps this is why physicists are more vocal than biologists in promoting a pacific solution of the territorial dispute between science and traditional philosophy. At the present moment it is the fashion among those who are writing on scientific philosophy either to neglect the contribution of the biologist to the world symposium, or to assume that the biologist in dealing with living matter operates with different methods and different concepts from those employed in physics. No phase in the history of biology is more fitted than the present to illustrate the fundamental unity of scientific method. In no branch of science is the limit of applicability of scientific method a more significant issue.

Since a scientific concept is only a way of describing a class of properties, the nature of life cannot refer to anything but the nature of the properties of living things. Having arrived at some general classification of the characteristic properties of living things, a discussion of the nature of life in the light of modern biological science presents two issues of pre-eminent interest. One is how far the *methods* employed in physical science have been successful and are likely to continue to prove successful in dealing with the properties of living matter. The other is how the increasing measure of success which attends the utilization of purely physical *concepts* to interpret the properties of living matter is calculated to influence our evaluation of the place of science in human thought. Whatever differences of interpretation may exist among biologists on matters of detail, it should at least be possible to infer from a survey of the progress of biology whether the study of living matter is progressing satisfactorily along the lines of quantitative analysis of experimental data towards greater certainty of prediction, and whether there is good reason to believe that the preservation of the teleological standpoint in dealing with living matter is likely to ensure conspicuous success in the same direction.

It may be admitted that there exists among biologists more unanimity with reference to the first than towards the second issue. Every infant science makes use of notions peculiar to its own province. Chemistry has but lately passed beyond the stage when the concept of *affinity* first became amenable to interpretation in thermodynamical quantities. There are still many biologists who would assert that the concept of *adaptation* demarcates the province of biology from that of physics and chemistry by an impass-

able gulf. There are others, fewer in number, who, surveying the teleological growing pains of the more exact sciences and bearing in mind that only 300 years have passed since the properties of familiar chemical compounds were literally personified as spirits of wood, spirits of salt and the like, do not feel compelled to regard the concept of adaptation as final. They are able to entertain the possibility that those properties which enable an organism to maintain its continued existence as an organism are not permanently more incapable of physical interpretation than the polarization of a voltaic battery, a phenomenon which the consistent teleologist would presumably regard as an attempt on the part of the latter to save its own life. Clearly the onus of defining what precisely is implied in the concept of adaptation lies on those who assert its uniqueness. Until the vitalist is more definite on this issue, the mechanist is under no obligation to refrain from classifying the properties of living matter in the light of his own experience. The mechanist denies that anything is to be gained by clinging to the teleological standpoint with its implication of some extra- or intra-mundane purpose which has been abandoned in all branches of science that lay claim to exactitude. He refuses to deal with living matter except in as far as it is considered as a series of " events " whose characteristics must be interpreted with rigid economy of hypothesis.

In approaching any lump of living matter, let us say the author of these essays, as an object of the external world, the maintenance of economy of hypothesis compels the enquirer to seek as far as possible a common basis for the characteristic properties of living and non-living systems. This necessitates a clear definition of the distinction between the two. Taking a comparatively complex organism, as,

for instance, the common frog, a distinction might be attempted along the following lines. In the first place, its possibilities of behaviour are more varied than those of any machine which can be manufactured by man; yet, while possessing a greater range of reversible response than any non-living system, it would be difficult to specify in a living system any single activity which could not be reproduced by a mechanical system. Apart from this diversity, which we may refer to under the generic term *reactivity*, living matter is characterized in general by the wide range of external influences which are significant in determining its characteristic reversible responses. This peculiarity, in view of the subjective preconceptions implicit in the older terms irritability, sensation, etc., may be denoted by the term *receptivity*. Here again it is impossible to isolate any single agency (or "*stimulus*") capable of evoking reversible change in any living system and incapable of evoking reversible change in any non-living system. Finally—and at first sight—a more diagnostic difference between living and non-living matter is seen in the property of *reproduction* (taken in the broader sense of the term, to include growth). A given piece of living matter comes into being in our experience only through the agency of other pieces of living matter closely resembling itself.

§ 2

Were the more obscure process of sexual reproduction universally characteristic of living matter, this distinction would appear especially fundamental. Experimental biology is far from the achievement of a complete physico-chemical analysis of asexual reproduction in any type of organism. On the other hand, in the life cycles of those multitudes of

micro-organisms which multiply by simple fission after attaining a certain limit of growth, there is nothing which compels an unprejudiced investigator to regard the process as more intrinsically incapable of physical interpretation than the splitting into two of a liquid drop. Although our knowledge of the nature of sexual reproduction is fragmentary, in this very field some particularly spectacular advances have been registered in substituting physical agencies as effective instruments for initiating processes which at one time were only amenable to the influences of living matter itself. Thirty years have now passed since Loeb's discovery that changes in the osmotic pressure of the external medium or alteration of the permeability of the egg itself, leading to changes in its own internal osmotic pressure, can initiate without any assistance from the sperm the development of the ovum into a new and complete organism. That discovery was the starting-point of a body of investigations whose influence has radiated into many other fields of biological enquiry. Especially noteworthy in this connexion is the work of Warburg during the last decade. Warburg was able to show that sea-urchin eggs, and later animal cells in general, if rapidly dehydrated and ground to a powder, will, like the intact cell, absorb oxygen for some time when moistened. He showed also that this property, like respiration in the intact cell, can be abolished by the action of cyanides and other classes of tissue poisons. By doing so, Warburg has taken a characteristic and highly complex property of living matter out of the realm of vitalism into that of physical chemistry. His analysis went further. Experiment showed that three classes of poisons which inhibit tissue respiration can be distinguished by their quantitative relations. Of these, the

efficacy of one class, the cyanides, was shown by Warburg to be correlated with the iron content of the cell. On the hypothesis that iron catalysis is the main factor in the oxidation of organic material in the cell, Warburg manufactured suspensions of charcoal with a high iron content capable of catalysing the auto-oxidation of sugars, fats, etc. The catalytic activity of these suspensions was found to be related quantitatively to the three categories of respiratory poisons in a manner closely parallel to the action of the latter on tissue respiration.

Though reproduction is, in some respects, to the biologist at least, the most fundamental of all the three features which I have defined above, the everchanging reactivity and manifold receptivity to external influences so characteristic of living matter pre-eminently engage our attention in connexion with the more intimate and subtle issues of a field of enquiry which biology may yet claim. I refer to the analysis of human behaviour. In this connexion I shall mention progress in three directions as illustrating the transition from teleological to quantitative treatment during the last half century; namely, the physical analysis of the events which constitute an isolated unit of response or reflex, the integration of reflexes in the normal behaviour of animals, and the determination of new behaviour patterns along the lines laid down by Pavlov's school. With regard to the first, we will consider the effect of flashing a bright light upon an animal that has been previously kept in the dark. The characteristic response, let us say, blinking of the eyelids, and the intervening events involved are, first, a physical change in a receptive area, namely, the retina; secondly, the propagation of the disturbance there set up along a certain path, the nervous

system ; thirdly, the liberation of a considerable quantity of energy at the seat of response or effector organ, that is to say, the muscles of the eyelid.

Our knowledge of the nature of receptivity is least complete. That it is a measurable physical event is beyond dispute. When light impinges upon a given area of the retina there follows a characteristic series of changes in electrical potential of the excited area with reference to a non-excited area. Through the work of Jolly, Adrian and others the sequence, the time relations and the magnitude of these changes are being related to the intensity and duration of the stimulus within predictable limits for a given species. These events initiate the propagation of the disturbance known as the nervous impulse. The nervous impulse is a physical event whose space-time relations can be defined as concretely as the passage of an electric current through a wire. Three-quarters of a century ago Helmholtz showed that the time which elapses between the application of a stimulus to a nerve and contraction of its attached muscle is a linear function of the distance between the latter and the point of application of the stimulus. The conception of the nervous impulse as a physical event had been, till this discovery, entirely repugnant to scientific thought. We now know not only, as Helmholtz showed, that the nervous impulse has definite space-time co-ordinates, but that it has the dimensions of energy. Its passage corresponds to the rate of propagation of an electrical change of an analogous character to the electrical response of the excited retina. The total energy of its propagation has been recently measured by Gerrard and A. V. Hill from determinations of heat production during its passage. Its mass relations are attested by a measurable increase in the

carbon dioxide production of stimulated nerve. The rate of propagation of the nervous impulse varies like all chemical reactions in a characteristic way with increase in temperature. The goal of the nervous impulse after it has traversed one or more synapses in the central nervous system is the effecter organ itself—in the case of blinking of the eyelids, a muscle fibre. During the past two decades a series of brilliant researches based on calorimetric methods have revolutionized our knowledge of the final component of the reflex. A. V. Hill and Meyerhoff have correlated the chemical and energetic changes accompanying muscular contraction with a precision of the order expected in purely physico-chemical determinations. They have shown that the total energy of muscular contraction can be quantitatively related to the energy liberated *in vitro* by the breakdown into lactic acid of an amount of glycogen equivalent to that which is converted into lactic acid in the actual contractile process.

Passing from the analysis of the constituent events of the reflex to the integration of reflexes in normal behaviour, we are faced with a striking change in the attitude of enquiry adopted in the study of those aspects of behaviour determined by generalized stimuli such as light and gravity and denoted by the term *tropisms*. Three-quarters of a century ago, after Helmholtz had dispelled the belief that identified the nervous impulse with an imponderable psychical principle, biologists like Lubbock were content for the most part with the statement that the moth flies towards the candle because it likes the light. The work of Loeb and others has shown that the state of contraction of particular groups of muscles is reflexly determined by the stimulation of particular areas of the retina. It is a mechanical necessity that when different areas are unequally stimulated,

differences in tension of different groups of muscles will bring the body into such a position that symmetrically opposite areas will be equally illuminated. The animal must move, as in fact it does, along the path of the incident beam, whether by so doing it brings itself into a brighter, or, as can easily be arranged experimentally, a darker situation. Whereas the older and purely teleological attitude permits us to predict nothing of consequence, the objective interpretation of tropisms by experimental methods permits us to make many verifiable predictions, as, for instance, the fact that the moth will move in circles, if one eye is blackened, owing to the fact that the muscles on that side will be more relaxed.

By the end of the nineteenth century, experimental biologists were generally disposed to the belief that the analysis of the reflex and the integration of reflex systems were problems not of apologetics but of energetics. Investigation had been confined to those aspects of behaviour which are for practical purposes invariable responses to a particular situation. From the human standpoint the most fascinating feature of the behaviour of an organism is, after all, the extent to which its behaviour is conditioned not by the immediate but by the antecedent situation. In the opening years of this century the researches of Sherrington were elucidating the integration of reflexes in normal behaviour. Restricted as they were to the decerebrate animal, the traditional distinction between reflex and voluntary activity remained as a defeatist formula in biological nomenclature. The distinction was not a gratuitous olive branch to introspective philosophy. It had its objective basis in the domain of behaviour which is not uniquely determined by the immediate stimulus, when all synchronous

conditions have been standardized. That distinction has been superseded to-day by the work of Pavlov's school, which has shown, first, that in the higher animals with the cerebrum intact, new reflex systems can be built up experimentally under perfectly definable and reproducible conditions; that the relations between such conditioned reflexes can be defined in the language of space and time; and that the concept of sensation can be externalized by reference to the ability of a given stimulus to become a specific agent in the building up of a new reflex system. In short, it is legitimate to anticipate the possibility of giving a complete specification of how such an animal as a dog will behave in a given situation without recourse to the traditional nomenclature of memory, consciousness, sensation, etc.

Pavlov's work is now accessible to the English reader through two translations of the Russian physiologist's own writings and several excellent résumés, such as the one given in Lovatt Evans' *Recent Advances in Physiology*. How far-reaching are its consequences has not been widely recognized even by biologists themselves. Experimental biology, during its brief career, has attempted to accommodate itself to the introspective temper of traditional philosophy by a compromise explicitly formulated in the writings of Descartes, who bequeathed to physiology the dualism of mind and matter. In conformity with the Chaldæan mythos, many philosophers, Descartes among them, have endowed Man alone with soulfulness. The coming of the Evolutionary hypothesis has broken down so inflexible a distinction between Man and other forms of living matter. Evolutionists in the nineteenth century, like Haeckel, were prepared to equip the Amœba with a soul. In our time the Cartesian compromise has again shifted its

boundaries. By the beginning of this century the moth once more had gone to join the candle. Still Man stood with a little family of mammals around him, each with one leg on either side of the frontier that separates the universe of space and time from the Platonic world of universals. Pavlov has taken those aspects of behaviour which would have been referred twenty years ago to exclusively introspective concepts, and has treated them successfully as predictable configurations in a space-time framework. The little family of mammals has been let through the tollgate of the Cartesian compromise. A new school of psychologists has come into being with the express object of making psychology a physical science, relieving Man, the celestial pilgrim, of his burden of soul.

Philosophers have always had a legitimate cause for complaint that biologists were unable to deal with those aspects of human life which interest people most. The distinction between reflex and voluntary activity provided the fullest absolution for that amiable libertarianism which we all entertain under the influence of alcohol and love. Because that distinction was implicit in the outlook of the most radical mechanists of the last generation, Loeb among them, Dr Haldane finds it so easy to point out the inadequacy of the mechanistic outlook. In the light of Pavlov's work we can now envisage the possibility that the methods of physical science will one day claim the whole field of what can be properly called knowledge. If I am right in cherishing such an opinion, it would thus appear that the investigation of the conditioned reflex initiates a new epoch in biology, pregnant with more far-reaching philosophical implications than the evolutionary speculations of the nineteenth century. The fact that no reference

to the conditioned reflex is contained in Dr Haldane's Gifford Lectures may in part account for the fact that he can so easily dispose of the mechanistic position. The modern mechanist does not say that thought and love and heroism do not exist. He says, show me behaviour to which you apply the adjectives thoughtful or loving or heroic, and we will, one fine day, endeavour to arrive at predictable conclusions with reference to it by following the only method of enquiry which we have learned by experience to trust. When Dr Haldane goes out of his way to dispose of the puerile formula that thought is a secretion of the brain, as bile is a secretion of the liver, and does so, I gather, under the impression that mechanists either believe it to mean something or alternatively shut their eyes to the major problems of existence, I can only respectfully suggest that he is flogging a dead horse, while the living ones are getting out of the vitalistic stables.

I have endeavoured so far to indicate the increasing measure of success that has crowned the application of physical methods and the use of physical concepts in modern biological investigation. I have attempted to illustrate the continuous retreat from teleological concepts that has accompanied this advance. In asking what progress may be anticipated from encouraging the teleological attitude to the nature of life, I wish now to urge that the important advances of biological science during the last hundred years have not only involved continual abandonment of teleological concepts, but have consistently been made in the teeth of opposition from the vitalists, organiscists and holists of their time. A century ago, in the same year that witnessed Wöhler's announcement of the successful synthesis of Urea, the great chemist Henry wrote (1827) concerning

organic compounds: "It is not probable that we shall ever attain the power of imitating Nature in these operations. For in the functions of a living plant a directing principle appears to be concerned peculiar to animated bodies and superior to and differing from the cause which has been termed chemical affinity." Only six years before Helmholtz's determination of the velocity of the nervous impulse in 1851, Johannes Müller had declared that to measure the propagation of that imponderable psychical principle was a theoretical absurdity.

It is not unlikely that before another celebration of the centenary of Wöhler's achievement, Fischer's synthesis of an octadecapeptide will have been surpassed by the manufacture of complex proteins in the laboratory. Looking forward a little in the light of what success has crowned the construction of physical models of vital processes, it is, as Sir Edward Sharpey Schafer scandalously suggested at a meeting of the British Association some years ago, perfectly legitimate to entertain the possibility, even the likelihood, that scientists will one day construct from artificially synthesized organic materials, systems with so wide a range of reversible reactivity and receptivity to external influences that they would be called organisms, if met with in Nature. While taking a more hopeful view in this matter than some biologists, I would remark that the validity of the mechanistic outlook is quite independent of this possibility. The security of any dynamical system of treating the motions of the heavenly bodies is independent of the possibility that human effort could manufacture a new satellite for Jupiter.

§ 3

If we can assert that the present phase of biological enquiry is a peculiarly fruitful one, and that there is no reason to see any immediate cessation of progress in the use of physical concepts as the basis of our analysis of the properties of living matter, can we not go further and state that we have absolutely no encouragement for entertaining the hope that any deeper knowledge will accrue from apostrophizing under the sobriquets of entelechy, life force, élan vital that elusive entity to which, perhaps, the poet William Blake referred as Old Nobodaddy ? It is doubtful whether we shall see a recrudescence of such frankly animistic devices as these. As biology becomes more technical and more exact, an aptitude for rehabilitating oriental mysticism in somewhat unusual verbiage will be regarded as an insufficient equipment for entering the field of biological controversy. The investigator who abandons physics for the pursuit of biology will contribute new ideas. Fruitful contributions need no longer be expected from those who combine the pursuit of literature with an amiable interest in natural history. The days of Butler and Bergson are passed.

Dr Haldane, the most vigorous contemporary critic of the mechanistic standpoint, is very anxious to avoid any suspicion of being tainted with the cruder forms of vitalism. He disowns any allegiance to the life force, élan vital et hoc genus omne, except in so far as he, somewhat mysteriously, contrives to introduce an adventitious deity into the latter portion of his Gifford Lectures. This does not make its appearance until his major thesis is complete. Anxious as is Dr Haldane to disclaim adherence to the

tenets of vitalism, he is very definite in denying the possibility that atomistic concepts will ever successfully deal with the problem of what he calls "conscious behaviour". In the light of Pavlov's work we see that the problem of what is usually called conscious behaviour, or as we should rather say *conditioned behaviour*, can now be approached as a problem in the study of those conditions which determine whether a new reflex will, or will not, be brought into being. We may state this in other words by enquiring how the passage of impulses along particular tracts in the central nervous system *influences the facility with which the nervous impulse will pass across a particular type of synapse.* Since the problem of the conductivity of the synapse is, as we have seen, an essentially physical problem, it is not overstating the case to say that the work of Pavlov's school has brought the study of what Dr Haldane calls "conscious behaviour" within the realm of physical enquiry. Once this is fully grasped it no longer seems incredible that the interpretation of conditioned behaviour will eventually come within the scope of physico-chemical analysis. Contrary to the holistic standpoint, we are thus led to an atomistic concept of individuality. This I shall venture to formulate as *the statistical probability that in an immensely elaborate system of reversible reactions a certain number of states characteristic of any given moment will be reproducible at another moment.*

In his Lowell lectures Professor A. V. Hill lays down two general conclusions derived from the extension of modern biological enquiry. First, as I have endeavoured to show, there is no limit to the extent to which the mechanisms of life can be elucidated with the aid of physical methods and concepts. Second, that, however far we get,

we shall still find function, adaptation, organization and purpose in the processes we explore. I would venture to suggest that, however alluring such a compromise between vitalism and mechanism may appear, these two conclusions, though formally in nowise inconsistent, are, nevertheless, in practice incompatible. As Henderson points out in his *Fitness of the Environment*, if we wish to indulge in teleological phantasies, we can find as much scope in physics and chemistry as in biology. We do not dismiss the hypothesis that thunderstorms occur when a blue unicorn sneezes on Uranus, because it is actually possible to disprove so engaging a fancy, but simply because other ways of treating thunderstorms lead to more useful conclusions.

Hence it seems to me that as we come to understand more and more about the mechanics of living systems by using methods of which Professor Hill is so brilliant an exponent, we shall inevitably find ourselves talking less and less about purpose and function. In consequence many of the problems which now engage the attention of philosophers will be relegated to the same status as the philosopher's stone. No doubt such a change will come very gradually, so gradually that we shall hardly notice it. Nevertheless, one may venture to predict that philosophers, already forced by the developments of modern physics to divert their attention from the pretentious crossword puzzles of the Hegelian tradition, will sooner or later be driven to take account of the post-evolutionary developments in biology, and more especially those which have their starting-point in Pavlov's researches.

By undertaking the analysis of the characteristics of conscious behaviour without departing from the methods of the traditional physiology of reflex action, biological science

in our generation has shown that there is no nicely defined boundary at which physiology ends and moral philosophy begins. Hitherto physiology and academic philosophy have developed independently, because physiologists themselves have accepted the common sense dualism of mind and matter. Moral philosophy can no longer claim that there is any distinctive aspect of the Nature of Life, which lies beyond the province of physiological enquiry. If any fundamental distinction between mind and matter remains, that distinction henceforth defines the antinomy of a *public world* of common beliefs which all can share, the conceptual world of science in which ethical neutrality and economy of hypothesis reign supreme, and, in contradistinction to that public world, many *private* worlds which for the present remain impenetrable through the medium of discourse. Biological science is continually socializing our beliefs. What seems irrevocably part of the private worlds of one generation becomes irrevocably part of the public world of its grandchildren. Thus the new pluralism will not be, like the Cartesian system, static, but dynamic. It is ever tending towards a monistic outlook as a limiting case. Such a monism, unlike traditional materialism and traditional idealism will be regarded not as a formula but as an asymptote. It is evidently immaterial to *public* discourse whether we *privately* entertain the view that *the* public world is more or less *real* than *our* private worlds. It would thus seem that as biological science invades the province of human behaviour the concept of *publicity*, as I venture to call the communicability of beliefs, will come to occupy the status of importance which *reality* has held in the systems of egocentric philosophers.

The public world, as I have conceived it, is a construction

based on the continuous extension of the principle of mechanism. The principle of mechanism, that a complex system is interpretable only by reference to the properties of its constituent parts, is not urged in the spirit of dogmatic assertion, but because it has served us well in the past. We still await any single verifiable conclusion that is uniquely developed from any alternative principle. Holism, the newest form of Vitalism, claims to have found an alternative or supplementary principle that is essentially teleological. The holist does not specify by reference to any single concrete situation how he proposes to use his principle. It is admitted even by the mechanist that we are not in a position to construct a symbolic relation which will completely describe the vagaries of a Ford car in terms of the field equations of the proton and electron. Does the holist wish us to believe that we can help anyone to drive a car by assuring him that at every level of complexity between the internal structure of the atom and the newly licensed automobile there emerges an ever-increasing urge to a wholeness or unity which is somehow indefinably different from the interaction of the parts ? Verily the mechanist of all people knows that we know in part and we prophesy in part. For this very reason, because he is prepared to await with patience the slow advance of science, he refuses to subscribe to high-sounding pseudonyms for ignorance and principles that are never seriously intended to be put into practice.

In the recent symposium on *The Nature of Life* before the British Association both General Smuts in his exposition of the holistic standpoint and Dr Haldane who supported him dwelt upon the supposed collapse of mechanistic principles in physics itself. The former cited in support of his

point a somewhat rhetorical remark by Dr Whitehead in this sense. It is of course evident that if our mechanical principles undergo modification our biological interpretations must share in the general change of outlook. It is, therefore, beside the point to criticize the mechanistic standpoint on the ground that our mechanical principles are undergoing revision. Let us examine this objection a little more closely. Experimental biology, we are told, has been directed towards the attempt to describe the properties of living matter in terms of the traditional physical concepts of mass, length, time, energy, etc. Since these concepts now appear to be less fundamental than we once believed, the hope that a complete mathematical description of the universe is realizable, has, as Mr Sullivan asserts with triumphant *naïveté*, " no longer any plausibility ". Surely it is evident that a signal advance towards a more monistic interpretation of nature has been made, when the analysis of any biological phenomenon has been achieved with the aid of traditional physical concepts, and when concepts once peculiar to biology, as affinity was once peculiar to chemistry, have been translated into the traditional language of physics. Physics to-day is seeking a new synthesis to take into one system of equations all the old data, and many new ones which have lately accumulated. This is not a new situation. The old mechanics remains as valid as ever for the realm in which it was developed to operate. To effect a more comprehensive scheme it has been necessary to examine many of the old postulates. In the meantime we have to recognize that we are not so near to a single unifying hypothesis as the rise of energetics led the physicists of Kelvin's generation to hope. What does this signify ? Certainly not that mechanics has abandoned the

principles of mechanism. Is it not rather a fact that the modern physicist is complaining that the inadequacy of Newtonian principles is in part attributable to teleological implications insufficiently recognized till now? The very hope of finality which Kelvin's generation entertained seems from the new mechanistic standpoint, as I have stated it, to savour of scholasticism.

In taking this line General Smuts and Dr Haldane seem to me to have laid bare the source of a misunderstanding that lies at the root of most of the criticism which vitalists old or new direct against the new or the old mechanistic standpoint. Those who have the scholastic predilection for finality and the scholastic predilection for the abstract noun, do not seem to be able to believe in the existence of people who are not like themselves. They cannot, it appears, understand that unless one starts off with the obsession that the universe can be summed up in a monosyllable, one is under no imperative necessity on the one hand to be resentful towards or disappointed with science because it lays no claim to the finality of religious dogma, nor on the other to make the assumption that such finality ought to be obtainable. The mechanist does not claim that his system is, or ever will be, complete in the sense that science will one day find an answer for all the conundrums which the scholastic temperament dictates. On the contrary, it is the essence of the mechanistic position that there is a technique of asking questions profitably as well as a way of answering them satisfactorily. All the mechanist claims is that as far as we can see at present his way of dealing with things leads to the most complete unanimity which it is possible to attain. Against the old vitalism, that of Dr Haldane, who denies that the principle of mechanism can

ever deal with conscious behaviour the older mechanistic outlook was secure in the assurance that, if the principle of mechanism failed at such a level, no other principle led to verifiable predictions in the same field. Against the new vitalism or holistic standpoint of General Smuts which no longer asserts dogmatically that the principle of mechanism is inapplicable at any specific level of existence, but contends that it does not anywhere give a complete account, the new mechanistic or *publicist* standpoint which I have outlined contends that if the principle of mechanism fails to give a complete account at any level no alternative or supplementary principle has been discovered. The reply of the mechanist old or new to the vitalist old or new is that of Mr W. B. Yeats' faeries:

> "Is anything better, anything better
> Tell us it then . . ."

It follows that, in any discussion between the two, the combatants are generally at cross-purposes. The mechanist is primarily concerned with an epistemological issue. His critic has always an ontological axe to grind. The mechanist is concerned with how to proceed to a construction which will represent as much about the universe as human beings with their limited range of receptor organs can agree to accept. The vitalist or holist has an incorrigible urge to get behind the limitations of our receptor organs and discover what the universe is *really* like. What we mean by *really* in this connexion evidently depends upon whether we view the question socially or individually. In our relation to other human beings the nearest approach to what the universe is really like is found in the schematization of our common experiences. If there is any other reality its

sanction is non-social. Thus in contradistinction to the *reality* of traditional philosophy which is an individualistic concept, the concept of *publicity*, which it is proposed to substitute as the goal of synthetic philosophy, is an essentially social one.

IV

THE CONCEPT OF ADAPTATION

"No philosopher who is rational and modest has ever pretended to assign the ultimate cause of any natural operation, or to show distinctly the action of that power which produces single effect in the universe. It is confessed that the utmost effort of human reason is to reduce the principles productive of natural phenomena to a greater simplicity. . . . The most perfect philosophy of the natural kind only staves off our ignorance a little longer, as perhaps the most perfect philosophy of the moral or metaphysical kind serves only to discover larger portions of it."—David Hume, *Sceptical Doubts*

§ 1

By those who hesitate to commit themselves to an explicit advocacy of either the vitalistic or mechanistic views about the Nature of Life it has often been urged that the concept of adaptation is fundamental to biological science. Professor A. V. Hill is perhaps the most brilliant physiologist now living. He adopts a hopeful attitude to the progress which awaits further analysis of the properties of living matter in physico-chemical terms. He also thinks that, however far mechanistic principles are extended, the biologist will always encounter "adaptation" in the phenomena which he studies. Another distinguished physiologist, Professor Lovatt Evans has expressed himself in rather more emphatic terms.

"Physiologists", he states, "in attempting to know what life is, have in my opinion attempted too much, and I think that a new point of view is essential. . . . The idea of adaptation, urged by Claude Bernard, should be adopted by physiology as its basal principle, as the chemist accepts the

conservation of matter or the physicist the conservation of energy. We need not seek to know why it is so, that is the province of the philosopher. . . . It is not a definition of what life is, but a brief statement of its way. . . . Life is conserved by adaptation."

When I first read these words I was not sure that I agreed with them. I was not quite certain that I knew what they meant. I had already come to the conclusion that the word adaptation is frequently used by biologists without a very clear agreement as to its content. I cannot subscribe to the view that there is a sort of trade union of philosophers to which physiologists are ineligible, unless they can show their articles of apprenticeship. Nor can I conceive what is meant by a concept of life except such as is implied in a statement of *its way*. A scientific concept defines a class of properties. A scientific concept of life or adaptation must conform to this requirement. In this essay my object is not to criticize Professor Lovatt Evans for whose breadth of view I entertain a very sincere respect. I have quoted his words, because they focus attention on some significant and controversial issues. They serve to reveal how imperative it has become that biologists should agree about the sense in which they intend to use the word *adaptation*.

The quotation given above might be interpreted to mean two very different things. If the term adaptation is used to define certain very general characteristics of living systems, it becomes almost co-extensive with a scientific concept of life itself. If we use *principle* in a somewhat archaic sense to indicate a field for investigation, like the principle of affinity or the active principle of the thyroid gland, there can be no question that the idea of adaptation is the basic principle of physiology. The comparison of the biologist with the chemist or physicist seems to go beyond this, and

imply that adaptation is not something to be explored and interpreted, but part of the logical procedure of biology, something by the aid of which we can predict conclusions of universal validity in the field of biological enquiry. I do not think that Professor Lovatt Evans really means this. I do urge that biologists continually confuse within the compass of the concept of adaptation the notion of a problem for solution and of a *vera causa*. This in everything but verbiage is precisely what the cruder type of vitalist does, when he invokes the vital principle. He first introduces a term to describe a large number of things about which we are ignorant and wish that we knew more. He then falls into the trap of imagining that the invention of a new term has solved the problem.

Quite apart from this difference which, if it is to define the scope of our scientific enquiries, cannot be dismissed as metaphysics, biologists differ a good deal concerning the extent of the phenomena and the kinds of phenomena they are dealing with, when they speak of adaptation. The physiologist—in the restricted sense of the term—is usually referring to something which might be called the self-regulating characteristic of the body. The evolutionary biologist—who to-day is a physiologist in the broader sense of the term—is usually thinking of " a change in the structure, and by implication also in the habits of an animal which render it better fitted " for life. I here quote Professor D. M. S. Watson's suggestive address on adaptation from the evolutionary standpoint.[1] Sometimes the word adaptation has a more comprehensive significance and includes both definitions which I have distinguished. It then amounts to saying that living systems are self-regula-

[1] British Association 1929, Section D.

ting and self-propagating, which is one way of defining the nature of life as a scientific concept. None of these technical uses of the word adaptation imply anything that the most dogmatic mechanist could decry. If we define adaptation as the self-regulating processes by which living matter retains its recognizable characteristics, it is a truism to say that life is preserved by adaptation. In that case, if adaptation is to be made the paramount issue for biological enquiry, we can hardly upbraid our predecessors for presumptuously seeking to know what life is. If we are to reach any agreement about the use of the word adaptation we must therefore retrace our steps, and examine more closely what are the characteristics of a living system. It is useless to define the goal of biological enquiry in terms of a concept which is as vague as life itself. I suggest that when, in its various uses, the term adaptation has any objective utility, it refers to these two more or less distinct categories of characteristics which living beings display, i.e. self-regulating and self-propagating. They are separable issues inasmuch as a worker bee and a Dominican friar are self-regulating but not self-propagating systems. There is no particular reason to object to the use of the prefix, so long as no personalistic implications of the word self are imposed upon it without further discussion.

§ 2

Of the two ways in which the word adaptation is used in biological discussion, that which implies the notion of self-regulation is most fundamental. A living organism is an extremely complex system in dynamic equilibrium with its environment. The idea of dynamical equilibrium is not peculiar to biology. The atom, which for traditional chemistry was a statical concept, is no longer regarded in

that way by the modern physicist. What is more peculiar about living matter is its amazing complexity, and the idea of adaptation in the sense of self-regulation calls attention to the fact that a system of such extreme complexity, a system with so many characteristics, continues to maintain its individuality, i.e., its manifold characteristics, in spite of all the changes that are taking place within it and without. The recognition of this complexity is common ground. If the mechanist underrates the difficulty of the problem, he is certainly to be discouraged, except in so far as the scientist in attacking any problem must always focus his attention on a limited range of data and rule out certain things as insignificant for his present purpose. It may be true, as Professor Lovatt Evans opines, that mechanistic interpretations tend to become arrogant and superficial. Is he on surer ground in holding that "it is unthinkable that a chance encounter of physico-chemical phenomena can be the explanation"? Might we not reflect with David Hume that "our own mind being narrow and contracted, we cannot extend our conception to the variety and extent of nature, but imagine that she is as much bounded in her operations, as we are in our speculation"?

Scientific hypotheses are not always thinkable, if by that we mean pleasant, easy or conformable to common sense. Our grandfathers thought it "inconceivable" that her Gracious Majesty, Queen of Great Britain and Ireland, Empress of India and Defender of the Faith, could be descended from an ape. The atomic structure of matter was unthinkable to many people little more than a century ago. To-day the quantum atomic model is unthinkable; but we think it is the best way of interpreting the data. Given this

amazingly complex system in dynamically stable equilibrium with its environment, we have to decide consistently with the fullest requirements of the problems what is the most economical way in which we can envisage its existence. Seeing that a mechanistic interpretation is evidently the most economical one, the real issue is to decide whether there are any characteristics of the complex which are inconsistent with such an attitude.

From the modern standpoint the individuality of the atom is a statistical concept. The atom is in dynamically stable equilibrium with its surroundings. It might, therefore, be argued that a Ford car is an example of a complex mechanism which is in dynamical equilibrium with its environment. This would be a superficial analogy for the order of complexity which we encounter in living matter. The molecular constitution of the parts of a Ford car is comparatively static. In the minutest parts of an organism new molecules are being built up and replacing others that have been broken down. Nevertheless, in all this astonishing panorama of microscopic revolutions which underlie the microscopic continuity of the organism we know of no events which are in conflict with the great generalizations of physical science.

On this point Professor Hill speaks with special authority, when he declares :

> " Fortunately for physiology several of the generalizations of science appear to be fairly strictly true, even when applied to the living organism. Although such exact experiments are not possible on man, or animals, or plants, as may be made on non-living objects, there is little evidence—indeed, I would be bold and say there is no evidence—that such living creatures can, in any manner or degree, evade the ordinary laws of mechanics, chemistry and physics, the principles of the Conservation of Energy and Mass. . . . There really is

> *no* evidence that momentum and kinetic energy, that chemical transformations, that electrical and magnetic phenomena, occur in the living body in any manner, or to any extent, which differs from that obtaining in the more readily investigated non-living world."

In the same lecture Professor Hill replies to a statement which has been frequently reiterated by vitalistic writers including General Smuts and Professor Julian Huxley. Referring to the Second Law of Thermodynamics, he says :

> " Philosophically speaking, the Second Law of Thermodynamics, dealing with the limitations of the availability of Energy, is more liable to doubt. It is known to rest on a statistical basis, and when we are dealing with units, complete, self-producing, yet as invisible and intangible as the filter-passing or other micro-organisms, it is, theoretically speaking, possible that some means may be available of evading the statistical relations which govern the behaviour of larger systems. But here again we must ask for evidence—and there is none of a precise or definite character which suggests, in the least degree, that the living cell can escape the jurisdiction of the Second Law."

We are thus forced to consider the order of complexity of the living system maintained in dynamical equilibrium with its surroundings as the essential feature which distinguishes it from non-living things. This complexity can be arbitrarily divided into many levels ; but for convenience we may confine ourselves to two, the macroscopic and the microscopic. Let us be explicit about the meaning of this distinction. In the more familiar animals, we are accustomed to recognize a variety of responses to a variety of external agencies. Generally speaking in the more complex animals each kind of reactivity and each kind of receptivity is spatially localized. For instance, light impinging upon the retina evokes contraction of the pigment cells in the toes of a frog. From this macroscopic complexity of the

gross architecture of the animal body arise two types of problems : first, the problems of co-ordination dealing with the way in which a disturbance recurring in some receptive area is propagated to an effector organ (gland, muscle, etc.) in some other region ; and second, the problems of metabolic exchange, dealing with how the supply and distribution of sources of energy for all this display of activity is maintained. The first involves the study of the nervous impulse along the peripheral nerve fibres and through the central nervous system ; it also involves the study of the internal secretions. The second involves the study of digestion and assimilation of foodstuffs, the intake of oxygen to burn up the waste products of chemical activity, and the removal of carbon dioxide, water and other products of oxidation. In contradistinction to the gross complexity of organs or populations of cells, we have to take into account the microscopic complexity of the cell itself. This presents a more general issue, because there exist many organisms whose complexity is of the same order as that of the separate cells which make up the bodies of familiar animals of visible dimensions. Two of the major problems of cell physiology concern the way in which the cell maintains its semi-permeability, and the way in which it maintains a constant renewal of chemical materials by utilizing the energy liberated in certain organic oxidations.

If we remove the magneto from a car, we can keep it intact for an indefinite period : it is fundamental to our idea of a mechanism that it can be taken to pieces and put together again. We are so accustomed to think of a leg or an arm as dependent for their activity on the rest of the body, that the conception of a living mechanism is repugnant to common sense. In the laboratory it is possible to

study properties of nerve, muscle, the cell membrane, absorption of food in the gut, oxidation of nitrogenous materials in the liver, etc., as isolated events. A person who is not a biologist almost invariably expresses bewilderment when he sees the isolated heart of an animal beating regularly in a perfusion apparatus. There exists the idea that the living organism differs from a mechanical system in that the parts cannot persist without the whole.[1] Behind this illusion of common sense the holistic concept of adaptation stands securely entrenched. The holistic conception implies that for living systems the part must be interpreted in relation to the whole, and not the whole from the interaction of parts. We have seen that the ultimate non-biological constituents of living matter, molecules, atoms, etc., do not behave differently when united to form a living system. In transcending this level of organization we are faced with an equally striking conclusion. The contraction of an isolated muscle preparation is essentially the same as the contraction of a muscle considered as an isolated aspect of the behaviour of the intact organism. The passage of the nervous impulse along an isolated nerve is not fundamentally different from the passage of the nervous impulse in the normal animal. The conversion of sugar into alcohol by the isolated enzyme zymase obtained from crushed yeast cells is a process like that of the conversion of sugar into alcohol by the living yeast fungus. The whole development of physiology, from the time when Haller first made an isolated muscle preparation and Spallanzani produced animal light by moistening a dessicated powder of luminescent jelly fishes, bears witness to the conclusion that the

[1] Professor Wildon Carr, in defending the vitalistic standpoint, explicitly states this, as quoted in a later essay.

separated constituents of a living whole do not at any level of complexity behave differently from the way in which they behave as parts of a more complex order. Thus, when the fullest recognition is given to the extreme complexity of living systems, the problem of self-regulation submitted to experimental analysis does not bring forward any confirmation for the holistic view of adaptation as the creative interpolation of new irreducible properties at different levels of complexity. The holist may reply that it is one thing to take the living machine to pieces, and another thing to put it together again. Even here the analogy with the machine holds good. To graft the eye of one salamander tadpole on to the head of another individual is now a commonplace of experimental embryology. Five-legged and two-headed newts are now manufactured in the laboratory.

Self-regulation, the way in which an organism maintains a seeming continuity of arrangement in spite of the uninterrupted and ubiquitous flux of macroscopic and microscopic changes which its existence implies, defines the sense in which the term adaptation is ordinarily used by the physiologist. In contradistinction to this physiological and individual use, adaptation is employed in biological discussion in a morphological and specific sense, when we consider how one animal comes to be distinguished from another by some architectural arrangement appropriate to a particular kind of environment. In this sense the problem of adaptation has played a prominent part in the evolutionary speculations of the past century. Given the fact that organisms are not only self-regulating but self-propagating, the evolutionary theory sets out to explain how living systems come to exist in so many specific forms, and how it is that these specific forms are on the whole *fitted* or *adapted* to their

respective surroundings. The qualification *on the whole* is highly significant. Organisms display many peculiarities of architecture which by no stretch of imagination can be regarded as necessarily fitting them better for their conditions of life. To assume that every peculiarity of structure in an animal is useful to it in the struggle for existence is a pure assumption unfounded on anything but teleological prejudice.

Adaptation in the morphological sense really includes two ideas which to some extent coalesce, and are therefore all the more readily confused. At times the word implies nothing more than *viability*. In this sense adaptation is the whole problem of evolution. Up to a certain point an organism must be "suited" to its environment in order to live at all. At other times adaptation is extended to mean an essential utility in every detail of the structure of an organism. This is a mischievous implication which, as will be seen later, has hindered the formation of a clear conception of the evolutionary process. Even if we could justify the belief that female peafowl are as much impressed by peacocks as are some male biologists, we have still failed to supply a criterion of survival value which has any satisfactory significance. The enthusiast who describes an adaptation is often like the advertizing manager who tells us how many customers we shall get, if we advertize with him, but is inclined to be reticent about whether the profit derived from more customers is commensurate with the fees he proposes to exact for his services. Bateson, who first applied Mendel's principles to animals, wrote five years before the Mendelian Renaissance in terms which may still be commended to the thoughtful examination of every student of the evolutionary problems:

"Whereas the only possible test of the utility of a structure is a quantitative one, such a quantitative method of assessment is entirely beyond our powers. To know that the presence of a certain organ may lead to the preservation of the race is useless, if we cannot tell how much preservation it can effect . . . unless we know also the degree to which its presence is harmful, unless, in fact, we know how its presence affects the profit and loss account of the organism." (*Materials for the Study of Variation*).

That animals do in fact display many structural characteristics which are in no sense useful to them is generally admitted to-day. It thus becomes as much the function of any theory of the evolutionary process to explain the origin of useless as to explain the origin of useful devices. There is a practical limit to the use of the concept of adaptation in morphology. There is a no less obvious limit to the use of the concept of adaptation in physiology. An animal is a self-regulating system up to a point; but it cannot in every contingency take arms against a sea of troubles and by opposing end them If we could define in some general terms where this limit lies, we should be justified in speaking of a principle of adaptation in the sense that we speak of a principle of conservation of matter. The ideally self-regulating unit of living matter endowed with the secret of perpetual youth is as imaginary as the Economic Man. At present the fact that organisms cease to regulate themselves and die is as fundamental a problem of biology as the converse fact that they regulate themselves and thereby continue to live. The fact that the organism has a good deal of useless anatomical equipment seems to be as true as the fact that on the whole its anatomy is suited to the requirements of its surroundings. In whichever way we employ the term adaptation we are forced to the conclusion that it is only legitimate to speak of a

principle of adaptation in the sense in which we speak of the active principle of the thyroid gland. Adaptation defines a field of problems which await solution. In that sense the concept of adaptation is as fundamental to mechanistic as to any other theories of the organism.

§ 3

This is not what is generally meant when it is said that adaptation is a fundamental principle of biological enquiry. I believe that it is the only legitimate sense in which it can be said that there is a biological principle of adaptation. It seems to me that, when we go further and put more than this into our concept of adaptation, we are driven to formulating the problems of biology in a wrong way. By inventing hypotheses to explain facts which do not exist, we then proceed to give false interpretations of the significance of facts that do exist.

When the principle of adaptation is treated as a principle which enables us to predict conclusions, it constantly leads us to fantastic distortions of what really happens. If I wished to illustrate this in connexion with the self-regulating aspect of the concept of adaptation, I could not do better than refer to current speculations about the rôle of the ductless glands in the economy of the organism. The physiologist who interprets his field of observation in a manner analogous to that of the physicist and chemist realizes that we have no reason to believe that every chemical entity found in the animal body is necessary or even useful to its owner. He will not therefore draw any conclusions of a far-reaching nature from the discovery that a certain tissue extract has highly specific physiological properties, unless he can show that the removal of the tissue itself

produces effects of an opposite nature to those which ensue on injecting its active constituent. The student of ductless glands who regards adaptation as a principle to be applied rather than as field to be explored will not be held back by such restraint. We must thank the "principle" of adaptation in endocrinology for the romantic guess-work of that school which undertakes to interpret the whole of human history in terms of a glandular explanation of temperament. Most speculations on which the glandular theory of temperament are based have their only experimental basis in the presence of supposedly specific active substances in one or other tissue extract. A "principle of adaptation" does not assist us to understand why the pituitary gland of a fish should contain one specific constituent which produces expansion of the black pigment cells in the skin of a frog, another specific constituent which causes the uterus of the mammal to contract, and yet a third which produces a specific fall of blood pressure in the bird and a specific rise of blood pressure in the mammal.

It is especially in the field of evolutionary biology that we must look for most guidance, because the concept of adaptation has occupied such a prominent part in the evolutionary controversy. As an example of how "the principle of adaptation" leads to incorrect conclusions I need cite only one example from an exceedingly able and provocative address of Professor D. M. S. Watson.

> "It is not unusual for a student of fossils to discuss the habits of an extinct animal on the basis of a structural resemblance of its 'adaptive features' with those of a living animal and then to pass on to make use of his conclusions as if they were facts in the discussion of an evolutionary history or of the mode of origin of a series of sediments. In extreme cases such evidence may be absolutely reliable: no

man faced with an ichthyosaur so perfectly preserved that the outlines of its fins are visible can possibly doubt that it is an aquatic animal, and such a conclusion based on structure is supported by the entire absence of ichthyosaurs in continental deposits of appropriate ages and their abundance in marine beds. But if extremes give good evidence, ordinary cases are always disputable. For example, there is, so far as I know, not the least evidence in the post-cranial skeleton that the hippopotamus is aquatic; its limbs show no swimming modification whatsoever, and the dorsal position of the eyes would be a small point on which to base assumptions. Most palæontologists believe that the dentition of a mammal, and by inference also that of a reptile or fish, is highly adaptive, that its character will be closely correlated with the animal's food, and that from it the habits of an extinct animal can be inferred with safety. Here again the extreme cases are justified, the flesh-eating teeth of a cat and the grinding battery of the horse are clearly related to diet. Crushing dentitions, with the modification of skull and jaw shape and of musculature which go with them, seem equally characteristic. I had always believed that the horny plates and the jaws of Platypus were adapted to hard food, and that that animal possessed them, whilst the closely allied Echidna was toothless, because it was aquatic and lived in rivers which might be expected to have a rich molluscan fauna which could serve as food. But the half-dozen specimens whose stomachs I have opened contained no molluscs whatsoever, and seem to have fed on insect larvæ, the ordinary soft bottom fauna of a stream." (*Op. cit.*)

We are now beginning to see that the evolutionists of the nineteenth century focused their attention far too exclusively on adaptation. In other words, they regarded adaptation as a principle like the principle of conservation of matter, one of universal validity within the field of biology. Any theory of evolution has to explain why non-adaptive, as well as adaptive, features arise. In that sense the fundamental problem of evolution is not the origin of adaptation but the origin of species. Both the theories of Lamarck and Darwin implicitly assumed that the differences between species, in the traditional, i.e. Linnæan sense, are mainly

utilitarian. Having started with an incorrect apprehension of the facts they proceeded to elaborate hypotheses to account for them. Thence inevitably they drew from these hypotheses an unsatisfactory account of the way in which new species do arise. From the modern standpoint analysis of the species problem does not demand a recognition that species differences are even in the main utilitarian, though such a statement would probably be true of differences between larger units such as genera. Nor from the modern standpoint do the hypotheses of either Lamarck or Darwin give us any clue to the way in which the species barrier, i.e. inability to breed with other species successfully, can have arisen. Anything which remains of the Lamarckian principle in the light of modern research has no special relevance to the origin of adaptations. Whatever remains of the theory of natural selection has been completely divested of the implication that non-adaptive characters were necessarily adaptive at their inception.

A discussion of the fate of the Lamarckian and Darwinian theories must be undertaken elsewhere. Here it is sufficient to point out that both, more particularly the latter, had a peculiarly sterilizing influence on the growth of experimental biology. Obsessed with the principle of universal adaptation which Natural Selection had secularized, zoology, from the publication of the *Origin of Species* to the rediscovery of Mendel's Laws, wandered for forty years in a wilderness of phylogenetic speculation. Biological research in the words of Professor Punnett became

> "devoted to the construction of hypothetical pedigrees suggesting the various tracks of evolution. . . . The result of such work may be said to have shown that the diverse forms under which living things exist to-day, and have existed in the past, so far as palæontology can tell us, are

consistent with the view that they are all related by the community of descent. . . . It is obvious that all this work has little or nothing to do with the manner in which species are formed."

According to the Selectionist doctrine in its original form, characters originated and persisted in virtue of their utility and what Darwin somewhat vaguely called "the strong principle of inheritance". To explain any peculiarity of structure or habit, it became necessary only to show one of two things, either it was useful to its owner or was once useful to an ancestor of its owner. Everything was or had been an adaptation. This resulted in a complete divorce of comparative anatomy from comparative physiology. The morphologist and systematic zoologist regarded it as an impertinence of the physiologist to seek for experimental evidence, where a perfectly good case of adaptation was evident to anyone who would accept their premisses.

I will illustrate this from a field in which I have myself carried out experimental investigations for twelve years. Writing of colour change in frogs Dr Hans Gadow makes the following remarks in the *Cambridge Natural History* (*Amphibia and Reptiles*, p. 36):

"Biedermann concludes that the chromatic function of frogs in general depends chiefly upon the sensory impressions received by the skin, while that of fishes depends upon the eye. All this sounds very well, but the observations and experiments are such as are usual in physiological laboratories, and the frogs, when absorbed in their native haunts, or even when kept under proper conditions, do not always behave as the physiologist thinks they should. There is no doubt that in many cases the changes of colour are not voluntary but reflex actions. It is quite conceivable that the sensation of sitting on a rough surface starts a whole train of processes: roughness means bark, bark is brown, change into brown; but one and the same tree frog does not always assume the colour of the bark, when it rests or when it sleeps upon such

> a piece. He will if it suits him remain grass green on a yellow stone or on a white window frame. . . . The sensory impression received through the skin of the belly is the same, no matter if the board be painted white, black or green, and how does it then come to pass that the frog adjusts its colour to a nicety to the general hue or tone of its surroundings."

It is safe to say that no one, unless at the outset prejudiced by the principle of adaptation, could be led to entertain the view that frogs as a rule are able to adjust themselves " to a nicety " to the general hue and tone of their surroundings. The state of the pigment cells in the skin is influenced independently by a number of diverse factors, including moisture, temperature, diffuse light acting on the skin and reflected light acting on the retina of the eye in the opposite sense. Individual frogs differ in basic pattern, but the range of hue between the dark and pale condition for any frog is fixed, as is also true of the proverbial chameleon. When the conditions affecting colour change in a frog are defined, it is possible to predict the pigmentary response of a frog and its time relations with as much confidence as any other physical event in nature. It is, on the other hand, quite impossible to draw any far-reaching conclusions about colour change from uncontrolled observation of the frog in its native haunts, because the number of significant variables is far too numerous to handle in this way. I have quoted this passage to show the attitude which zoologists under the influence of the post-Darwinian tradition adopted towards experimental enquiry of any description. Dr. Gadow applies the " principle of adaptation ", as it was then used in morphology, to the self-regulating aspect of the organism with results which show what might well happen to physiology if the physiologist employed

the principle of adaptation as the chemist employs the principle of conservation of matter.

Ecology, or the study of the relation of species to particular types of environment, provides a clear illustration of the progress that has been achieved by detailed study of physiological mechanisms in place of the speculative application of the principle of " adaptation ". Krogh and his pupils have made a special study of the physico-chemical properties of the blood pigments of the lower organisms, and have thereby thrown a good deal of light on the conditions which determine their ecological distribution. Let us take the case of two common bony fishes, the carp and the trout. It is a matter of common experience that in nature the trout will only live in running streams. It can be kept with great difficulty in aquaria, if special precautions for aerating the water are taken. The carp will live in still water, where the oxygen content is low, and like its ally the goldfish accommodates itself to the aquarium with great ease. The difference between the two types is at once understood, when we know that the hæmoglobin of the carp has a much higher affinity for oxygen than the hæmoglobin of the trout. In consequence the blood of the carp is completely saturated with oxygen when the oxygen content of the water in which it swims is far below that which is in equilibrium with the oxygen pressure of the atmosphere. The blood of the trout on the other hand is only fully saturated with oxygen when the water is itself nearly saturated.

The concentration of salts in the blood of fishes like the trout and carp is kept constant at a level below that of sea water. The concentration of dissolved substances in the blood of sharks and dogfishes which are all marine is in equilibrium with the osmotic pressure of the sea. The

respiratory centre of the wrasse is paralysed at 6° C., while the heart of the English dogfish shows irreversible changes above 18° C. Taking these facts together we can deduce a good deal about the viability of a species in a given locality. A fish like the skate placed near the estuary of a large river is forced to remain where the salt concentration is above a certain level. A salmon is not subject to this restraint. Assuming that the fish can pass the estuarine boundary and proceed upstream, two alternatives present themselves. He can remain in the swiftly moving main stream or take to backwaters and stagnant lakes connected with it. If he has the hæmoglobin of a trout, he is committed irretrievably to the former alternative. Being compelled to remain in the swiftly running part of the river bed, he might stay in the lowlands or make for the source, which in general will be much colder. In the case of a fish like the wrasse, whose respiratory centre is paralysed at a temperature of 6° C., the latter course is impossible, if the river rises in a high range. Thus in place of vaguely speculating about how an organism is specially "adapted" to live in some particular place, experimental biology is gathering clearly defined ideas about why an organism cannot live in any place other than that in which it does live.

The idea that a problem can be solved by invoking the principle of adaptation assumes its most grotesque form in Haeckel's discussion of Recapitulation. The classical example of what is called recapitulation is provided by the gill clefts of vertebrates. All vertebrate embryos have pits or clefts at the sides of the throat, supplied by a characteristic arrangement of blood-vessels. In fishes the clefts acquire filaments richly supplied with blood-vessels, and act as gills. Both the gill clefts and the characteristic

arrangement of blood-vessels associated with them persist throughout life. In frogs and salamanders gill filaments are developed in the tadpole stage, but the clefts disappear in adult life and the characteristic arrangement of their blood supply becomes profoundly changed. In Man and most land vertebrates the gill clefts are never used as respiratory organs, and with their blood-vessels disappear at an early stage in development. During the first half of the last century Van Baer, the pioneer embryologist, propounded a generalization which may be stated thus: embryos of different species of animals of the same group are more alike than the adults, and the younger the embryo the greater are the resemblances. This generalization, well illustrated by the gill clefts, was later extended by Haeckel with the sonorous title "Biogenetische Grundgesetz". It is thus defined by its author: "The history of the fœtus is a recapitulation of the history of the race, or in other words, ontogeny is a recapitulation of phylogeny."

The way in which the modern geneticist handles the problem of development offers a striking contrast to the attitude of Haeckel and a generation of zoologists unduly preoccupied with the concept of adaptation. A recent investigation from the laboratory of Professor Julian Huxley will illustrate the difference. In the little crustacean *Gammarus* there are a number of varieties distinguished by the colour of their eyes. All coloured eyes are at an early stage of development colourless. They then become scarlet owing to the formation of a red pigment. They may subsequently darken owing to the deposition of the black substance known as melanin. Varieties with eye colour from a dark red through various grades of chocolate to dark brown and black are distinguished by the time at which the deposition

of melanin begins and the rate at which it occurs. Here there is no difficulty in seeing what conditions must be fulfilled in order that a new variety should or should not recapitulate the characteristic of the ancestral stock from which it arises. If a red-eyed variety of Gammarus arose from a white-eyed stock, it would necessarily exhibit the ancestral condition at the beginning of development, because all eyes are at first colourless. If a black-eyed form arose as a sport from a red-eyed stock, it would also recapitulate the ancestral characteristic, because all black eyes are at first red. If a white-eyed form arose as a sport in a red-eyed stock, or a red-eyed form emerged from a black-eyed stock, in neither case would the ancestral condition be manifest at any stage of development. There is no question of the intrinsic usefulness of a new character involved in this. Whether recapitulation does or does not occur here depends upon whether the Mendelian factor which distinguishes a new variety hastens or retards some feature of the developmental process.

Now Haeckel's "Grundgesetz" implies an additional statement to that contained in Van Baer's Law. It signifies that the embryonic stages of one form are to be compared with adult rather than embryonic stages of another. This in fact is not correct, as the classical cases of recapitulatory phenomena demonstrate most clearly. The mammalian embryo never possesses true gills. It goes through a stage at which it has the characteristic clefts and arterial arches which in the *fish embryo* precede the development of functional gills. This is also true of crustacean larvæ. *Sacculina*, the crab gall, passes through the two characteristic larval forms of the true *barnacles*. It has no resemblance to an adult barnacle in any stage. A more serious objection to

Haeckel's way of stating the idea of recapitulation in development is the vagueness it assumes when brought face to face with the exceptions that are as numerous as the applications of the rule. An illustration of the exceptions is provided by eye colour in the human species. It is fairly certain that the blue-eyed condition has arisen as a mutant in a brown-eyed stock ; yet the eyes of brown-eyed adults are often blue in the newly born.

It is not difficult to discover in Haeckel's own writings the train of reasoning which led him to distort the facts of development in stating the law which is often associated with his name.

> "The evolution of the fœtus (or ontogenesis)," states Haeckel, "is a condensed and abbreviated recapitulation of the evolution of the stem (or phylogenesis) ; is preserved by a constant heredity ; on the other hand, it becomes less complete in proportion as a varying adaptation to new conditions increases the disturbing factors in the development (or cenogenesis). The cenogenetic alterations or distortions of the original paligenetic course of development take the form, as a rule, of a gradual displacement of the phenomena, which is slowly effected by adaptation to the changed conditions of embryonic existence during the course of thousands of years. This displacement may take place as regards either the locality or the time of the phenomenon. The first is called heterotopism, the second heterochronism."

So naïve a combination of garrulous teleology and self-contradiction is characteristic of the hopeless confusion of thought which existed in evolutionary biology, while it remained dominated by the principle of adaptation. The larval "adaptations" should on the face of it recapitulate their ancestral story—and so on in endless regression. There is no intelligible meaning in Haeckel's explanation of the admittedly ubiquitous exceptions to his rule.

Haeckel's so-called Biogenetische Grundgesetz exerted a

profound influence on biology during the second half of the nineteenth century, and perhaps did more than anything else to divert zoologists from the study of activity to the pursuit of insignificant details of no conceivable physiological interest. Instead of furthering the development of zoology as an exact science, it substituted the construction of architectural mnemonics for the search after quantitative laws. With Haeckel's law is associated an interesting logical fallacy in the development of the argument for evolution. Huxley made a good debating point when he disclosed the embarrassing information that a bishop at one stage of the episcopal life cycle has gill structures like those of a fish. From the standpoint of formal logic the point is worthless. Only the atmosphere of religious propaganda which surrounds the birth of the evolutionary doctrine can explain the perennial reappearance of the contention that recapitulation constitutes an argument *sui generis* in favour of the doctrine of descent. If experimental breeding taught us that mutant forms recapitulate the characteristics of the stock from which they originate, the resemblance of developmental stages of present-day forms to adult organisms which existed in the geological past would constitute a special consideration in favour of regarding fossil remains as ancestral to contemporary animals. As yet experimental breeding teaches us no such thing. We do not find that a white-eyed fly originating as a sport in a red-eyed stock invariably has red eyes at any prior stage of development. Recapitulatory phenomena are difficult to explain on a theological basis, but they do not constitute a special argument in favour of the evolutionary alternative. To-day biologists are beginning to realize that evolution must furnish an explanation of specific differences which are not adaptive as much as specific dif-

ferences which are adaptive. With this change of outlook it is becoming possible to discuss the logical status of the evolutionary hypothesis without recourse to arguments which belong more properly to propaganda than to science.

PART II

DARWINISM AND THE ATOMISTIC INTERPRETATION OF INHERITANCE

SUMMARY

The failure to recognize that biology no less than physics is an ethically neutral science is a heritage of the evolutionary controversy. The doctrine of organic evolution evoked intense religious hostility in the middle of the nineteenth century. Biologists were compelled to fight for their right to speculate on their own lines. Forced into the forum as a propagandist the biologist gave less attention to the logical structure of the new theory than to its apparent implications for social philosophy. The ethical concept of progress became entangled in the evolutionary idea. In the writings of Herbert Spencer and the evolutionist philosophers Darwinism has left a lasting impress upon contemporary thought. Experimental biology in this generation has undertaken the task of reducing the problems of organic evolution to an exact science. This must necessitate a re-examination of many traditional biological concepts and many philosophical and sociological inferences which have been extracted from an earlier phase in the development of the evolutionary doctrine.

V

THE METHODOLOGY OF EVOLUTION

"Chemistry is not so far from physics as the generation before ours thought. Biology, through bio-physics and bio-chemistry, no longer stands aloof from the methods and procedures of physical science. And these new alliances cannot be made without modifications in the logical construction of the separate concepts upon which these various sciences previously took their stands. This is a task which laboratory practice alone cannot undertake."—*Dorothy Wrinch*

§ 1

From Aristotle to our own time biologists have been too preoccupied with collecting information about the extremely complex phenomena which they study to pay very much attention to the logical structure of the hypotheses they adopt. This not only tends to make controversy between the mechanist and vitalist barren, but also explains why much that has recently been written and said about evolution is both unsatisfactory and perplexing to the intelligent layman. Many of the views which gained well-nigh universal assent among biologists in the latter half of the nineteenth century have been undermined by the discoveries of the Mendelian renaissance. When the onlooker asks the biologist for a straightforward exposition of the present status of the evolutionary hypothesis, he is frequently met with the guarded statement that biologists are no longer so sure that they know *how* evolution occurred, but are more certain than ever that it *has* occurred. Such a statement might conceivably have a logically admissible meaning, though if so, it belongs to the category of things which were better

said otherwise. On the face of it, the layman has very good reason for wondering whether it means anything at all. It is logically permissible to say we know that common salt is soluble, but we do not know how it happens that common salt should possess this property. But evolution is not a simple property. It is a process. We cannot very well know of the existence of a process unless we can say in what the process consists.

The doctrine of evolution which deals with the way in which living matter has come to exist in the manifold forms which biologists call species is one which can only be placed on the same footing as the great generalizations of physics and chemistry, when it is examined from the experimental standpoint. From that standpoint the particular phase in the growth of the evolutionary hypothesis associated with the names of Darwin and Wallace has less significance than is customarily attached to it. From a purely experimental point of view Darwin and Wallace brought to bear on the discussion of the evolutionary doctrine nothing which their predecessors Buffon, Erasmus Darwin, Lamarck, St Hilaire, Goethe and Oken lacked. The importance of their work lies in the history of the controversy. Under Cuvier's influence biology had turned away from premature speculation to industrious study of the nature of species differences from every available standpoint. Darwin and Wallace brought together the fruits of the progress resulting from a generation of intensive research on such lines, and formulated the evolutionary problem in a much more explicit form than *les philosophes* were in a position to do. The particular answer that they gave to the problem they formulated is the least significant part of the contribution which Darwin and Wallace made to bio-

logical science. The biological world did not begin to examine the experimental implications of the selectionist solution until the rediscovery of Mendel's laws by Correns, de Vries and Tschermak, and their extension to animals by Bateson and Cuenot in the opening years of the present century. The Mendelian renaissance provoked considerable hostility from a generation of biologists untrained in experimental methods. It is only now becoming possible to re-examine the selectionist doctrine with detachment and candour.

It must not be implied that antagonism to the new movement was a mere disinclination to face the effort of learning new methods of attacking the problem. In the nineteenth century biologists had to fight for their right to speculate freely in their own field. The generation in whose memory the struggles of that period were fresh not unnaturally resented the suggestion that biologists were no longer unanimous among themselves. It was heresy to betray the policy of a united front. If such schisms were permitted, and the truth were allowed to leak out to the general public, the church somnolent might again become the church militant. The recently published biography of the late William Bateson shows how keenly this was felt. In the end hostility towards the new movement which followed the rediscovery of Mendel's work gave place to a comfortable compromise, based on the attractive device of inventing a word for human ignorance. This device is not peculiar to biological science. There were from the start physicists who entertained the most profound suspicion of the ether on that account. It was agreed to state that inheritance in animals and plants is of two kinds, Mendelian and non-Mendelian. Study of the former was to be encouraged

because it was useful to stock breeders, horticulturalists, and pigeon fanciers. The latter was the peculiar speciality of the evolutionist. Apart from that, the impenitent selectionist did not attempt to define exactly what non-Mendelian inheritance was. Its sphere was progressively encroached upon by the Mendelian variety, until nothing was left of it but a comfortable corner for those highly variable characters which were somewhat vaguely referred to under the term "quantitative inheritance", i.e. hereditary differences in size so subject to fluctuating variability in response to external conditions that they are only definable by reference to a statistical average for a particular inbred stock. Naturally experiment first turned to the analysis of clear-cut hereditary differences such as colour, where little trouble is requisite in standardizing external conditions, so that an hereditary difference will be apparent in the individual. Since mathematical analysis has been brought to bear on the study of size inheritance in such work as that of East and Jones, there can no longer be any justification for doubting that the atomistic conception of heredity which Mendel formulated covers the whole domain of biparental inheritance.

While experimental analysis was progressing towards a recognition of the universal validity of the Mendelian conception, the brilliant work of Morgan's school was leading to an exact theory of the inter-relation of genetical factors based on the observed behaviour of the chromosomes. Experiment now equipped with a definite criterion of genetic purity could assert that new forms do come into existence discontinuously in nature. It could state the conditions which determine whether a new genetic character will persist. When chromosome maps of several allied species of the

fruit-fly were constructed by Metz and Sturtevant seven years ago the whole discussion of the problem of species formation entered on an entirely new phase. To-day we must approach the discussion of evolution on the assumption that in Mendel's atomistic conception of the hereditary process must be sought the correct interpretation of how new characters, having come into being, may be transmitted to future generations.

§ 2

To appreciate at once the greatness and the limitations of Darwin's contribution to evolutionary thought it is essential to see the question in its historical perspective. Many of the steps which have led to the construction of the evolutionary hypothesis are now only of historical interest. Only with an understanding of the history of the doctrine is it possible to gain a clear idea of the logical status it occupies in scientific thought. In approaching it, one has to remember that the discussion of organic evolution aroused a good deal of prejudice from religious quarters, and that in consequence many issues, e.g. Recapitulation, which were not strictly relevant to a straightforward presentation of the problem occupied a prominent place in the controversies that raged around it. In forming an estimate of the present status of the evolutionary hypothesis, let us, as far as possible, eliminate these irrelevant questions, and deal only with the steps which have made a definite constructive contribution to the present state of knowledge. These may be treated under four headings: (*a*) The Principle of Biogenesis, (*b*) The Principle of Unity of Type, (*c*) The Principle of Succession, and (*d*) The Principle of Genetic Variation.

The Principle of Biogenesis is simply the recognition that animals and plants only arise in our immediate experience from other animals and plants through the process of reproduction. Linnæus accepted it in his doctrine of the fixity of species as generally true with regard to animals in the ordinary sense. Not until the middle of the nineteenth century did the work of Pasteur demonstrate its validity for micro-organisms. Linnæus and Ray were among the first to recognize the general truth of the commonplace that "like begets like". The Aristotelian influence which predominated during the Renaissance had lingered on until the beginning of the seventeenth century. The fascinating legend of the goose barnacle contained in the concluding passage of Gerrards' *Herbal* (1594) is illustrated by an actual woodcut of the Goose and its Barnacle Progenitor. The passage reads:

> "But what our eyes have seene; and hands have touched we shall declare. There is a small Island in Lancashire called the Pile of Foulders, wherein are found the broken pieces of old and bruised ships, some whereof have beene cast thither by shipwracke, and also the trunks and bodies with the branches of old and rotten trees, cast up there likewise, whereon is found a certain spume or froth that in time breedith unto certain shells, in shape like those of the Muskle, but sharper pointed, and of whitish colour, wherein is contained a thing in forme like lace of silke finely woven as it were together, of a whitish colour, one end whereof is fastened unto the inside of the shell, even as the fish of Oisters and Muskels are; the other end is made fast unto the belly of a rude masse or lumpe which in time commeth to the shape of a Bird; when it is perfectly formed the shell gapeth open, and the first thing that appeareth is the foresaid lace or string; next come the legs of the bird hanging out, and as it groweth greater it openeth the shell by degrees, til at length it is all come forth and hangeth onely by the bill: in short space after it cometh to full maturitie and falleth unto the sea, where it gathereth feathers, and groweth to a fowle bigger than a

Mallard and less than a goose having blacke legs and bill or beak, and feathers blacke and white, spotted in such manner as is our magpie. . . . For the truth thereof if any doubt, may it please them to repaire unto me, and I shall satisfie them by the testimonie of good witnesses. . . . The bordes and rotten planks whereon are found these shells breeding the Barnacle are taken up on a small Island adjoyning Lancashier, halfe a mile from the main land, called the Pile of Foulders. They spawn as it were in March and April; the Geese are formed in May and June, and come to fulnesse of feathers in the month after. And thus having through God's assistance discoursed somewhat at large of Grasses, herbs, Shrubs, trees and Mosses, and certain Excrescences of the earth, with other things moe, incident to the historie thereof, we conclude and end our present Volume, with this Wonder of England. For the which God's Name be ever honoured and praised."

The legend of the goose and the barnacle died a slow death, and many diverting citations might be added. That canny Scot, Sir Robert Moray, wrote concerning the mystery surrounding the reproductive habits of geese and barnacles so late as 1678 in the following words, which occur in a paper actually published in the *Royal Society's Transactions*. After describing the barnacle shells washed up on the coast of Scotland, he refers to their "little bill like that of a goose, the eyes marked, the head, neck, breast, wings, tail and feet formed, the feathers everywhere perfectly shaped and blackish coloured, and the feet like those of other water fowl to my best remembrance."

Writing in the middle of the seventeenth century Sir Thomas Browne states (*Vulgar Errors*, bk. 3):

"Concerning the generation of frogs we shall briefly deliver that account which observation hath taught us. By frogs I understand not such, as arising from putrefaction are bred without copulation and because they subsist not long are called temporariæ (Rana temporaria, the common frog),

nor do I mean the little frog of an excellent parrot green that usually sits on trees and bushes, and is therefore called Rananculus viridis (the tree frog) but hereby I understand the aquatile or water frog, whereof, we may behold many millions every spring in England."

Referring to the doubt expressed by the author of *Vulgar Errors* concerning Aristotle's belief that mice arise from putrefaction, Alexander Ross commented:

"So may one doubt whether in cheese and timber worms are generated; or if beetles and wasps in cow's dung; or if butterflies, locusts, grasshoppers, shell fish, snails, eels and such like be procreated of putrefied matter which is apt to receive the form of that creature to which it is by formative powers disposed. To question this is to question reason, sense, and experience. If he doubt of this let him go to Egypt, and there he will find the fields swarming with mice, begot of the mud of Nylus, to the great calamity of the inhabitants."

During the sixteenth century under the influence of Vesalius, Fallopius and Servetus experimental investigation liberated medicine from the paralysing tradition of Galenic teleology. The effect of this change of outlook became evident in the revival of natural history in the seventeenth century. Redi (1688) turns to experiment to decide whether maggots can be produced from putrescent meat, if flies are prevented from depositing their eggs on it. "Reason, sense and experience" were at length forced to capitulate to experiment. The comparative study of animal life after centuries of stagnation following the publication of Aristotle's Natural History entered on a new phase. So long as innumerable *ad hoc* accounts of the origin of species existed the general problem with which the evolutionary hypothesis deals could not be envisaged. Thus the work of Linnæus is the starting-point of the modern theory of evolution.

More than a century elapsed before the essential features common to sexual reproduction in all animals were understood. Leeuwenhoek, a Hollander, in 1668 had first seen the minute spermatozoa in the seminal fluid. A little over a century later the ingenious Abbot Spallanzani gave experimental proof that it is to the spermatozoan that the seminal fluid owes its fertilizing power. Only in 1879 did Hertwig and Fol independently observe beneath the microscope that only one sperm normally fertilizes one egg. Their observations were made on sea-urchins, but we now know that their conclusions are true for all animals. Thus the recognition that everything implied in the term inheritance has reference to the material substance of the egg and sperm, a concept fundamental to any exact theory of hereditary transmission, did not emerge with clarity till more than fifteen years after the *Origin of Species* was published.

The formal classification of organisms codified by Linnæus introduced a new era of intensive investigation into the character of species differences and so ushered in the great age of comparative anatomy. Thus we come to the second step in the historical development of the Evolution theory, the *Principle of Unity of Type.* This generalization was the special contribution of the school of French and German comparative anatomists whose foremost exponent was Georges Cuvier. The work of Linnæus gave a great impetus to the study of the structural differences between animals, at a time when anatomy like any young branch of knowledge was still dominated by teleology. Some instructive examples of the happy combination of piety and anatomy are given in the *Speculum Mundi* published by John Swan in 1635. In an old translation of Pliny the

Elder there occurs the following information about the elephant :

> "Their skin is covered with haire or bristle, no, not so much as in their taile, which might serve them in good steade to drive away the busie and troublesome flies (for as vast and huge a beast as he is, the flie haunteth and stingeth him), but full their skin is of crosse wrinckles lattiswise ; and besides that, the smell thereof is able to draw and allure such vermine to it, and therefore when they are laid stretched along, and perceive the flies by whole swarmes settled on their skin, sodainly they draw those cranies and crevices together close, and so crush them all to death. This serves them instead of taile, maine and long hairs. . . ."

This citation is not an isolated instance of the way in which a pagan philosopher could employ the study of natural history to justify the ways of God to men. During the Middle Ages the influence of ecclesiasticism reinforced the teleological attitude from which Aristotle's Natural History is comparatively speaking free. At a later date Deism had its scientific complement in a tradition which identified the pursuit of Natural History with Natural Religion. The first classifications were based on comparatively superficial points of resemblance. As the study of animal structure progressed in the two generations that followed the labours of Ray and Linnæus, it became increasingly evident that the teleological standpoint in comparative anatomy is inadequate. If animals had been specially designed to suit their conditions of life, it would be expected that the greatest degree of similarity would be found in animals pursuing a similar mode of existence. This is not what is actually found. On the contrary, as we make the greatest degree of similarity in structure the basis of our attempts to classify animals, our units of classification resolve themselves into collections of forms which show the greatest diversity of habit, locality,

diet, means of progression or anything else which might be significant from a purposive standpoint. Animals can be classified in groups based on striking similarity in architecture and development involving complex constellations of physiological units. Within these groups the utmost variety of habitat, climate, locomotion, nutrition, etc., are encountered. The underlying similarity of the bones of the limb and its musculature in a whale, a bird and an elephant, as contrasted with the limb structures of a beetle, a fish or a squid illustrate this conclusion. The whole study of systematic zoology bears witness to it. Van Baer extended the principle of Unity of Type to embryonic forms in 1834.

The importance of the principle of Unity of Type to the Evolutionary hypothesis lies in the attitude which it promoted. By discouraging the teleological approach to the diversity of animal life, it paved the way for a naturalistic investigation of the problem. The net result of the intensive study of comparative anatomy which progressed under the influence of Cuvier in France and Johannes Muller in Germany was also to show that the task of classifying animals in hard and fast categories is at all turns embarrassed by the existence of anomalous intermediate forms like the duck-billed platypus or the worm-like arthropod Peripatus. Thus biological thought was becoming more and more sympathetic towards the existence of a process of species modification. This tendency became more sharply defined as biology took the third great step in the development of the modern theory of evolution.

This step has been called the *Principle of Succession.* When the Law of Unity of Type first obtained recognition, many fossils were known, but geologists had not arrived at a general agreement concerning the order in which the

various strata had been deposited nor the magnitude of the time which their formation occupied. By the middle of the nineteenth century the modern doctrine ("Uniformitarianism") had gained assent. It now became apparent from studying the distribution of animals in space and time, that divergent forms which exist on the earth's surface to-day were preceded by widely distributed forms of a more generalized type in the past. The further we go back in the history of any group of animals, the less do we find the same pronounced differences as are displayed by existing members of the same assemblage. The differentiation of species is inferred from the record of the rocks to have been a continuous process in space and time. This doctrine in its modern form was explicitly put forward in 1855 by Wallace.

The masterly way in which Darwin marshalled the facts at his disposal in presenting this aspect of the case constitutes his chief claim to have made an enduring contribution to the Doctrine of descent. From ancient times, but more especially from the end of the seventeenth century onwards, the hard remains of animals were discovered and described. Shells of molluscs which only live in water were found far inland remote from lake, river or sea. Such relics were attributed by the current mythology of Christian countries to the deluge that overwhelmed the contemporaries of the Noah family. Sceptics like Voltaire, who ventured to offer more naturalistic hypotheses, were not more felicitous in their speculations. An exception must be made in favour of Xenophanes (B.C. *circa* 500) and the Arab physician Avicenna, who, it appears, recognized fossils as remains of animals formerly alive, and saw in them evidence of the existence of oceans where there is

now only land. A giant fossil salamander which occurs abundantly in the Upper Miocene of Switzerland, closely related to the Japanese salamander *Cryptobranchus japonicus*, was unearthed by Scheuchzer in 1726, and named *Homo diluvii testis*. The motto attached to the figure reads :

> Betrübtes Beingerust von einem alten Sünder
> Erweiche Herz und Sinn der neuen Bösheitskinder.

This has been translated :

> Oh sad remains of bone, frame of poor Man of Sin,
> Soften the heart and mind of recent sinful kin.

After the Renaissance it seems that priority in the recognition of fossils as remains of what were once living animals is due to Steno (1699), a Danish anatomist who taught at Padua. More than a century later, Cuvier's monograph on fossil remains initiated the epoch of systematic palæontology. The effect of the researches which it initiated was not felt till the Uniformitarian doctrine, i.e. the view that successive strata have been deposited by a continuous process, was generally accepted, mainly through the work of Lyell (1830). The impiety of this new geology promoted violent controversy. In the minutes of a meeting of the Geological Society of Great Britain in 1840, we are told that the retiring president, Dr. Buckland, " with a look and tone of triumph pronounced upon his opponents who dared to question the orthodoxy of the scratches and grooves of the glacial mountains the pains of eternal itch without the privilege of scratching " (*Hist. Geol. Soc. Lond.*, p. 142). By the middle of the nineteenth century geologists were universally convinced that the various strata of which the earth's crust is composed have been laid down in orderly succession during periods of time compared with which that

occupied by the history of human society is of negligible duration. Once this conclusion was accepted, the study of fossils received a new impetus and progressed rapidly under the leadership of men like Owen, Cope and his contemporaries. Students of fossils now began to compare the characteristics of animals in different geological epochs, and to elucidate evidences of a continuous succession of new forms of life transmitted to posterity in the record of the rocks. Out of their studies the principle of succession took shape.

The geological succession of animal and plant life is demonstrated by two features of the record. Many of the more highly specialized and successful groups of the present day are not found to have existed at earlier periods of the earth's history, and were preceded by forms which are intermediate between them and representatives of surviving groups that were already existent before them. It is also found that the earliest members of the great groups are usually found to be of a more generalized type of structure than existing types. Adequate material for drawing these conclusions is provided only by forms which have resistant structures, such as the vertebrates, shellfish and vascular plants.

Before we can fully appreciate the continuity of the geological record, we have to take into account the fact that the same animals are not found in all the different parts of the globe. One group of animals may be confined, like the kangaroos, to Australia; one group, like the monotypic order, in which the ant-bear is placed, to South Africa. If, then, we know that there existed in, let us say, the Chalk Age, a small mammal which was of a type so generalized as to form a link between the kangaroo and the ant bear,

it is most important to know whether the barriers of ocean that now separate Australia and South Africa were as impassable in those times as they are now; or whether this architypal mammal lived in a situation from which it could have access to both of these promising lands of settlement for its family. We are thus led to ask if the process of geological succession was a continuous one both in time and space.

To answer this question demanded a comprehensive survey of the existing distribution of animal life on the earth, perhaps the most significant contribution that Darwin and Wallace made to the evolutionary doctrine. In their writings the facts of geographical distribution, facts which were very largely based on their own first-hand observations, and not like their erroneous views upon heredity collected from the testimony of other persons, first received critical examination. They were forced to conclude that no amount of ingenuity could successfully interpret the geographical distribution of animals on a purely teleological basis. The habitat of different kinds of animals is not uniquely determined by their special suitability for the locality in which they occur. This statement is attested by many species that were at one time restricted to a very definite area. When introduced into other parts by man they have flourished phenomenally. A familiar instance is the introduction of rabbits into Australia. The facts about the geographical distribution of living and fossil species collected by Darwin and Wallace resulted in an extension of the principle of geological succession. This is especially associated with the name of Wallace. Wallace's law (1855) is stated briefly at the conclusion of the memoir entitled *On the Law which has Regulated the Introduction of New*

Species. "Every species has come into existence coincident both in space and time with a pre-existing closely allied species."

With the statement of this law and its confirmation by more carefully sifted and comprehensive data the positive contribution of the nineteenth century to the development of the modern theory of evolution ended. The picture of a progressive gradual differentiation of animal life, as it spread over different parts of the earth in successive geological epochs became a commonplace of the naturalistic outlook. It remained for the Mendelian renaissance to clarify the conception of this gradual differentiation as an outcome of the agency of natural generation. It must be remembered that the Principle of Succession is still only a step in the formulation of a theory of Evolution. We have still to ascertain what is the natural process by which this progressive differentiation has been effected. The principle of Biogenesis forces us to look to the reproductive process for the answer; but only experiment can arbitrate in this field. The weaving together of principles derived from anatomy, embryology and geology in the light that experiment throws on the nature of the reproductive process is necessary to the completion of the evolutionary argument.

Let us now examine how much we have proved up to this point. We have seen that animals only come into being in our immediate experience through the agency of natural generation. We have also seen that similarity which animals display in their hereditable properties must be interpreted primarily in terms of the hereditability of the properties themselves, and not in terms of a purposive agency. Finally we have found evidence of a gradual and accumulative divergence in the hereditable properties

of animals continuing over vast geological epochs. We have still to interpret this divergence in terms of the only agency through which living matter in our experience is brought into being. We are thus led to the fourth step in our argument, the enunciation of the *Principle of Genetic Variation.*

This states the experimental fact that units of living matter with new hereditable properties do actually come into being in the normal operations of the process of natural generation. In using the word *experimental* in this connexion we lay bare a sharp divergence of standpoint between Darwin's generation and our own. Darwin collected a good deal of information about the origin of domesticated plants and animals. This seemed to his immediate successors to constitute sufficient evidence for believing that new hereditable properties arise in nature. The development of Mendelian analysis has shown that this is far from certain. Unless we have studied the parent stock under experimental conditions which safeguard its purity, we cannot be sure that a new domesticated variety is anything more than a new combination of genetical characters already present in pre-existing varieties. In other words it may only have arisen through hybridization. We have now at our disposal a clear concept of genetical purity and well-defined methods for establishing the purity of a stock. The whole question has been placed on a new foundation during the past three years by the *artificial* production of *mutants* or sports by X-rays in pure stocks of the fruit-fly Drosophila reared under experimental conditions.

A further discussion of the Principle of Genetical Variation with special reference to the Selection doctrine will be undertaken in a subsequent essay, when the possibility of building up new varieties into the units which biologists

call species will be dealt with more fully. To return to the discussion of the logical status of the evolutionary doctrine, we may assume that the Principle of Genetical Variation is established. On this assumption we may state the conclusion of the foregoing survey in the following terms. Animals with new hereditable properties have appeared successively with increasing divergence of type in the past history of the earth. Animals only arise in our experience by reproduction from pre-existing animals. Animals with new hereditable properties can arise in our immediate experience by reproduction from pre-existing animals with different hereditable properties. It is therefore natural to conclude that the existing divergence of specific characteristics is the outcome of a natural process of generation operating over long periods of geological time.

§ 3

Darwin's generation was in the main satisfied with the evidence derived from domestication. This was embodied as an *argumentum ad hominem* in the Selection hypothesis. The immediate effect of Darwin's influence was thus, as Punnett has remarked, " to divert interest from the study of the origin of species " as an experimental issue. Zoology and physiology became divorced in Great Britain. One resolved itself into a Somerset House for the Animal Kingdom, tracing pedigrees on a purely armorial basis. The other tended to develop in association with narrowly clinical objectives, till the rise of modern experimental zoology in the twentieth century. In the light of modern research the Selection hypothesis presents some interesting methodological aspects discussed elsewhere. Let us here confine ourselves to the evolutionary hypothesis in broad outline.

There are two fundamental results of the present enquiry which must be emphasized in any discussion of the logical structure of the evolutionary doctrine. One is the necessity of distinguishing between the Principle of Succession and the evolutionary hypothesis itself. The other is the recognition that in the last resort the validity of the evolutionary hypothesis rests on the issue of experiment. The first of these may sound like a platitude. It is frequently overlooked. Presumably when a biologist says we are more certain than ever to-day that evolution has occurred, but less certain about *how* it has occurred, he really means that the enormous extension of our knowledge of fossils has placed the Principle of Succession on a much firmer foundation than it enjoyed in Darwin's time. The mass of new information about comparative anatomy now available is more than ever inexplicable on a crudely teleological basis and more than ever consistent with an evolutionary interpretation, if such an interpretation is permissible. But evolution is more than succession. It is the interpretation of succession in terms of genetical variation. If experiment does not justify this interpretation, in other words if we do not know *how* evolution occurred, it is evident that we cannot be more certain that it has occurred.

The critique of evolution is not exhausted by a logical analysis of the experimental postulates of the hypothesis, because the doctrine of succession is more than a question of fact. It also implies the validity of current geological doctrines, whose logical status lies outside our present enquiry.[1] What are ordinarily called scientific hypotheses may be classified in two categories according to the test of validity which is applied to them. They might be called

[1] I refer to the Uniformitarian doctrine.

respectively *prospective* and *interpretative* for lack of existing terminology which makes the distinction which is relevant to our present object. If the consequences of one or other of a set of hypotheses each capable of accounting for a given series of data are uniquely capable of yielding verifiable conclusions about other realms of our experience, we accept the hypothesis which leads us to the new and previously undiscovered fact. We do this even if, in the absence of the new fact, the hypothesis so verified is a *less economical* one than others which satisfied the original data but do not account for the new one. By *prospective* hypotheses I mean hypotheses to which this test is applicable. They are such as permit us to make verifiable predictions in other fields of experience. In everyday language they assist us to prophesy correctly about future events. They constitute a hierarchy of socialized beliefs. By their aid mankind has been permitted to construct modern civilization. They possess to a pre-eminent degree the quality of *publicity* defined in an earlier essay. Mendel's hypothesis and the kinetic theory of gases belong to this category. Some writers, among others William James, have tended to imply that all so-called scientific hypotheses are of this type. This is not so. There are hypotheses whose justification resides only in the fact that they conform to the requirements of economy of thought. Such hypotheses are accepted because alternative hypotheses are less economical. They are incapable of yielding any verifiable consequences which follow uniquely from them. It is these to which I refer by the term *interpretative* hypotheses. We construct them, not because they are practically serviceable to us, but because they are conformable with the intellectual requirements of a civilization which is the practical outcome

of the application of science. They share two pre-eminent characteristics of the prospective type, economy of hypothesis and ethical neutrality. The need for them resides in our curiosity. They represent one aspect of the secularization of human life and the obsolescence of animistic ideas. We construct them for their *philosophic* interest alone. The evolutionary doctrine belongs to this category.

Few biologists would admit so heretical a conclusion. They would argue that every new missing link whose discovery is almost daily announced in the press provides verification of the predictions of the evolutionary hypothesis. But there is a fallacy in this contention. The discovery of missing links is not a unique consequence of the evolutionary doctrine. It might be inferred from the Principle of Succession, even if the evolutionary interpretation of the Principle of Succession turned out to be incorrect. Given the experimental postulates of the evolutionary hypothesis as established facts, the evolutionary hypothesis does not belong to the same hierarchy of scientific generalizations as the kinetic theory of gases or Mendel's Law, because as yet we are not able to predict with the aid of it any unique consequences which can be made the issue of decisive tests.

There is an interesting consequence of these considerations, and one which has a more comprehensive significance. Biology deals with two kinds of relations : relations between living and non-living matter and relations between different kinds of living matter. The Mechanistic Conception of Life is a secular extension of experimental analysis of the former, just as the evolutionary hypothesis is a secular extension of experimental study of the latter. Both belong to the category of interpretative hypotheses in the sense defined

above. Why is it then that so many prefer the luxury of scepticism concerning the first issue, and resent the exercise of a suspicion of scepticism concerning the second? Perhaps the answer is that evolution has already become incorporated in the apparatus of what Robert Briffault calls custom thought. I do not think that the physiologist who adopts the attitude of Gallio towards the mechanistic conception of life, affecting to despise all mere philosophy, is consistent, unless he is prepared to dismiss the doctrine of Organic Descent in the same manner. I have yet to meet one who does. Evolution is a philosophy.

VI

THE PROBLEM OF SPECIES

"The effect of Darwin's *Origin of Species* was to divert attention from the way in which species originate."—R. C. Punnett, *Mendelism*

§ 1

In a letter to H. de Varigny dated November 25, 1891, Thomas Henry Huxley wrote: "I shall be very glad to have your book on Experimental Evolution. I insisted on the necessity of obtaining experimental proof of the possibility of obtaining virtually infertile breeds from a common stock in 1860. . . . From the first I told Darwin this was the weak point of his case from the point of view of scientific logic. But in this matter we are just where we were thirty years ago." In this passage Huxley explicitly draws attention to the fact that Darwin never came to grips with the historic problem of the Origin of Species, as it had been propounded by Linnæus. Three years later he is writing to acknowledge the receipt of Bateson's *Materials for the Study of Variation*, a book which laid the philosophical foundations of the present era of experimental enquiry into evolutionary problems. "I see," he notes, "you are inclined to advocate the possibility of considerable *saltus* on the part of Dame Nature in her variations. I always took the same view, much to Darwin's disgust, and we used often to debate it." Another thirty years passed by, and Bateson ventured to appeal to his contemporaries for a reconsideration of the traditional species problem in the light of the accumulated results of investigation based on

Mendel's methods. He was rebuffed by a veritable storm of criticism from Huxley's followers. Evolution had become Darwin, as geometry has become Euclid. Had Huxley been living, it hardly seems likely that he would have taken the same side as his devoted disciples in the controversy which ensued.

During the latter half of the eighteenth and the beginning of the nineteenth century biological science progressed towards a clear definition of the problem of Man's secular origin. This progress involved the rejection of many of the teleological concepts which had been current since the Middle Ages. In the light of recent advances in the study of inheritance and variation, we know that much of the evidence which seemed adequate for an understanding of the evolutionary process fifty years ago must be re-examined to-day and supplemented from other sources. The final court of appeal in the case for an evolutionary interpretation of the origin of species is experiment. Only experiment can place the Principle of Genetic Variation, i.e. the origin of new genetic types in the normal course of procreation, on a sure foundation. A detailed examination of the evidence for this conclusion is essential to a satisfactory examination of the logical status of evolution in the light of modern knowledge. Four separate issues suggest themselves for discussion in a critical enquiry into the experimental evidence for the Principle of Genetic Variation. We must first ask whether the origin of new hereditable types under experimentally controlled conditions is an established fact. We must then decide what natural agency ensures that new types having so arisen will be preserved. This leads us to ask if the appearance of new types is an occurrence of sufficient frequency to have accounted for all the diver-

gency of specific form that has come about in the interval of time which geology places at our disposal. Finally we are faced with the task of deciding how new genetic types can be segregated into the units which biologists call species.

First let us consider the origin of new hereditable types. Thirty years of controlled experiment on the lines suggested by Mendel's work has given abundant proof that from time to time there do arise in pure stocks individuals which have entirely new hereditable properties. Such individuals are called *mutants* or sports, a term used synonymously by some writers with the alternative word mutations. The word mutation was originally employed by De Vries in a somewhat different sense from that in which the term mutant is now used. It is preferable to avoid perpetuating this confusion.[1] A new phase in this aspect of the evolutionary problem has been initiated by the recent work of Muller. A controllable agency, exposure of parents to X-rays, has been shown to produce mutants in the fruit-fly Drosophila.

Darwin and Wallace are usually given the credit of first emphasizing the fact of genetical variation. A careful study of their works shows that they did not clearly apprehend the essential aspect of the problem or realize the imperative necessity of subjecting the issue to direct experimental test. When they spoke of variation they included both genetical variation, i.e. the production of mutants as defined above, and differences between parents and offspring which result from the influence of external agencies in early development. The small differences of which Darwin was

[1] This objection does not apply to the use of the word mutation in its strictly etymological connotation as a process in contradistinction to a type.

thinking were mainly of bodily rather than germinal origin. As such they have nothing to do with the problem of evolution unless, as Darwin himself did, we accept the Lamarckian doctrine. In the Introduction to the *Origin of Species* Darwin states his position thus : " Any being, if it vary in any manner profitable to itself, under the complex and sometimes varying conditions on life, will have a better chance of surviving, and thus be naturally selected. From the strong principle of inheritance, any selected variety will tend to propagate its new and modified form." What he meant by the strong principle of inheritance Darwin never states in exact terms. Experimental knowledge was not ripe. Biology was still in the phase of *a priori* reasoning from " common-sense " principles. That he did not distinguish between bodily and germinal differences is shown by the following passage from Chapter 3 of the *Origin of Species* :

> " *Variations, however slight, and from whatever cause* proceeding, if they be in any degree profitable to the individuals of a species, in their infinitely complex relations to the individuals of a species . . . will tend to the preservation of such individuals and will generally be inherited by the offspring. The offspring also will have a better chance of surviving, for of the many individuals of a species which are periodically born, but a small number can survive. *I have called this principle, by which each slight variation if useful is preserved, by the term Natural Selection.*"—(Italics inserted.)

§ 2

The second aspect of the problem of genetical variation, formulated above, is the special issue raised by the selection hypothesis of Darwin and Wallace. Mendel might perhaps more justly be given priority for clearly envisaging the essence of the problem.

> "Those", wrote Mendel, "who survey the work done in this department will arrive at the conviction that among all the numerous experiments made not one has been carried out to such an extent and in such a way as to make it possible to determine the number of different forms under which the offspring of hybrids appear, or to arrange these forms with certainty according to their separate generations or definitely to ascertain their statistical relations. It requires indeed some courage to undertake a labour of such far-reaching extent. This appears, however, to be *the only right way by which we can finally* reach the solution of a question the importance of which cannot be overestimated in connexion with the history of *the evolution of organic forms.*"

Mendel's method shows us that so long as they attain sexual maturity and bear offspring, new forms having once arisen, transmit their hereditable properties unchanged. The new hereditary type will sooner or later appear among subsequent generations in its original purity.

This prompts us to ask what chance a given mutant has of surviving to sexual maturity. The question demands serious consideration. We know that a very small percentage of animals that are born into the world do actually survive till the age at which reproduction is possible. It has been calculated that if all the progeny of a single female aphis (the green plant louse) survived in every generation the total of individuals produced in twelve generations would be 10^{22}. Since a single aphis is about a tenth of an inch long, this number would cover the face of the globe. Twelve generations in a family of aphids would appear in less than three years. Evidently the chance that a given mutant will survive depends on two things. One is whether it possesses any characteristics which favour its survival in preference to the parent form. The other is whether it appears once or many times in the same stock. We now know that the same mutants appear again and again. There

seem to be definite *loci of instability* on the chromosomes. Which of these two considerations is of greater importance is at present problematical. Most biologists incline with good reason to regard the former as more significant. The significance of the second is increasingly realized.

Many contemporary authors use the term Natural Selection to imply that competition for the means of existence permits some mutants to live, and weeds out others. On grounds of priority this can hardly be regarded as justified by the writings of the Selectionist writers of the nineteenth century. It is not supported by the actual words Darwin used to define the term Natural Selection which he himself introduced. Goodrich in his admirable book entitled *Living Organisms*, makes the following statement with regard to Darwin's position:

> "It is often said that of late years Darwinism has lost ground, and that natural selection cannot be regarded as a satisfying explanation of, or even as an important factor in, the process of evolution. Doubtless there is some truth in the saying, at all events in so far as it appears that the doctrine is not what some misguided enthusiasts may have represented it to be, that it does not explain everything, that many problems remain unsolved. Yet the Darwinian theory still stands unassailable as the one and only rational scientific explanation of evolution by 'natural' forces whose action can be observed, tested and measured. Nevertheless, the critics are quite right in demanding convincing evidence for every step in the argument. The modern developments of the study of hereditary and variation on Mendelian lines, far from weakening the case for natural selection, seem to have definitely disposed of the only rival theory, the doctrine of Lamarck, founded on the supposed 'inheritance of acquired characters'. Fortuitous changes in the inherited organization, in the complex of factors transmitted, are left as the only elements of primary importance, the only stones of which the edifice is built."

These remarks imply that Darwin's successors went much further than Darwin in asserting the creative, preservative, accumulative and continuous character of the selection process. This is true; but Darwin himself, in the *Origin of Species*, expressly stated what he meant by Natural Selection in two quotations which have already been given; and neither of these agree with what Goodrich or any modern geneticist means when he says that he believes in natural selection. Since Darwin introduced the term he has priority in defining its meaning. If later biologists mean something different, when they speak of natural selection, it would avoid confusion to coin a new term. Elsewhere Goodrich says: "What selection alone can do is to preserve variations"; and he quotes Darwin's words in support. Darwin meant by *preserving* variations something different from what a modern geneticist believes. The modern geneticist believes that *individuals* who possess certain advantageous characters will survive in virtue of these advantages. Darwin and Wallace meant that *hereditary characteristics could only survive* if the supposed tendency to dilution of characters by crossing were counteracted by the elimination of individuals at the other end of the scale of variability.

Apart from what Darwin himself said on the subject we owe some consideration to the sense in which his contemporaries understood his argument. Since I may be accused of tilting with a lance of straw at a windmill of my own construction, let us refer to the section on swamping in Wallace's *Darwinism*. "He (Darwin) had always considered that the chief part and, latterly, the whole of the materials with which natural selection works was afforded by individual variations or that amount of ever-fluctuating variability

which exists in all organisms and in all their parts . . . " Wallace then proceeds to quote Romanes as saying that " if a sufficient number of individuals were thus simultaneously and similarly modified, there need no longer be any danger of the variety becoming *swamped by inter-crossing* ". Wallace himself wrote as follows :

> " I have already shown that every part of an organism in common species does vary to a very considerable amount in a large number of individuals and in the same locality ; the only point that remains to be discussed is whether any or most of these variations are ' beneficial '. But every one of these consists either in increase or diminution of size or power of the organ or faculty, that varies. . . . If less size of body would be beneficial, then as half the variations in size are above and half below the mean or existing standard of the species, there would be ample beneficial variations."

The implication is that natural selection by cutting off the other half—the ample non-beneficial variations—prevents the swamping of the beneficial ones out of existence. We know to-day that the traditional belief in the swamping effects of intercrossing is false. With its rejection the *argumentum ad hominem* which made the struggle for existence an essential agency for preserving new hereditary properties becomes unnecessary.

However much importance Darwin himself attributed to this aspect of his theory of Natural Selection, he makes clear his attitude in several passages. It cannot be doubted that the assent which he received from his contemporaries was in large measure due to it. Accepting the prevailing misconceptions about swamping, he showed how an evolutionary process could and, as it then appeared, must operate. Experimental evidence for the hereditability of the kind of variations on which Darwin seems to have relied was not brought forward. We now know that the kind of

variations which Darwin regarded as the raw materials for the selective process are not generally hereditable. The wisdom of retaining the term Natural Selection may therefore be questioned. In all probability there is another reason which in part explains the popularity of Darwin's theory as contrasted with the neglect of Mendel's pioneer labours. Natural selection was suggested by the analogy of industrial conditions in the nineteenth century. Once formulated as a universal principle of nature it appealed to the dominant political theories of the period. The Origin of Species became the bible of *laissez faire*. It triumphed as classical humanism triumphed during the Middle Ages in part at least for reasons which were primarily political. The idea that the struggle for existence is a constructive process played a prominent part in the social theories of the Selectionist School.

A great deal of confusion can be dispelled if we recognize that Darwin never clearly distinguished between two distinct issues. His herculean labours in the field of geographical distribution urged him to seek a reason for the circumstance that different species of animals exist in different parts of the world. The struggle for existence does explain why some species have died out in one place while others have died out in other places. From that point Darwin went on to generalize about the Origin of Species, i.e. how species come into being. It is unfortunate that, though most of his earlier and enduring contributions to science are concerned with how certain species have ceased to exist, the title of his work laid emphasis on the process by which species are brought into being. It was naturally this part of his theory which made the greatest appeal to his contemporaries. Possibly it was not the one which

was most significant to Darwin himself. Darwin used the term Natural Selection in connexion with both problems. With regard to the former his theory is as acceptable as ever. With regard to the latter it has now been superseded by exact experimental enquiry into the mechanism involved in the production and preservation of new hereditable types. The work of Gregor Mendel is the proper starting-point of such enquiry.

A third aspect of the Principle of Genetic Variation concerns the adequacy of geological time. It will only be touched on briefly. When the evolutionary theory was introduced to the biological world, it had to encounter a difficulty that no longer presents itself as a formidable objection. Kelvin had calculated the possible period of time during which life can have existed from considerations derived from the rate of cooling of the earth. The allowance which Kelvin conceded was subject to the qualification that no factors at that time undiscovered enter into the question significantly. Since that time the discovery of radio activity has removed the necessity to place any such restriction on the period of geological time, as Kelvin was led to deduce. To-day we have no reason for believing that geological time is too short to permit us to ascribe the faunistic changes of successive generations to the operations of the natural process of genetical variation. At the same time the Evolution Theory will not stand side by side with the retrospective hypotheses of astronomy in the hierarchy of scientific generalizations, until the frequency of genetical variation and the conditions which determine it have been correlated with more exact knowledge of the duration and climatic features of the intervals corresponding to geological strata.

§ 3

There remains another aspect of the Principle of Genetic Variation. This is of paramount importance in connexion with the evolutionary hypothesis. It is the Origin of Species *sensu stricto*. A good deal of confusion has arisen in the discussion of the species problem on account of the equivocal usage of the word *species*. It is therefore best to begin with a clear definition of the species problem. It is a universal experience that any dog resembles its father and mother in more respects than it resembles any cat or any fish; any cat resembles its father and mother in more respects than it resembles any dog or any fish; any fish resembles its father and mother more closely than it resembles any cat or any dog. We may express this by saying that cats, dogs and fish have certain specific hereditable properties. If we examine these hereditable properties we find that a cat has more hereditable properties in common with any dog than those which it shares with any fish. Thus organisms can be arranged or classified in groups expressing the extent of resemblance in their hereditable properties. The work of Ray and Linnæus in the early half of the eighteenth century led to the general belief that "like begets like", and the publication of the *Systema Naturæ* (1757) by the latter author marks the beginning of a century and a half of detailed anatomical studies directed to classification of this kind. According to the degree of resemblance of organisms with respect to their hereditary properties they are customarily grouped in phyla, classes, orders, families, genera and species. To illustrate the meaning of these terms let us consider the reader of this essay. He or she is said to belong to the species *sapiens* of the genus

Homo, which includes all living races of man. The genus *Homo* includes in addition *H. Neanderthalensis*, the early stone-age heavy-browed first men, and is grouped with the genera *Pithecanthropus* and *Eoanthropus* (the fossil ape man of Java and Pilt Down man) in a family *Hominidæ*, within the order *Primates*, that comprises apes, monkeys and marmosets. The order *Primates* is one of many orders of forms within the class *Mammalia* that includes hairy animals that suckle their young. The *Mammalia*, along with birds, reptiles, amphibia (frogs, toads salamanders) and fishes, is placed in the phylum *Vertebrata*, which includes all forms with a backbone.

The degree of similarity implied by placing two species in the same genus, order, class, etc., is an arbitrary one defined by convenience and general consent. The degree of similarity implied in placing two individuals in the same species in the sense in which the term was defined by Linnæus implies something more than convenience. Linnæus placed within the same species all individuals which breed readily with one another. The structural difference between two Linnæan species of animals and plants may be negligible compared with the immense structural differences that distinguish varieties within a single Linnæan species, as for instance the difference between White Leghorns, Yokohamas, Silkies, Partridge Cochins, etc., which are all members of the species *Gallus domesticus*.

Though this definition of the species as a unit is the one sanctioned by priority, it is insufficiently emphasized by those who discuss evolution that the creation of new species in the daily routine of a large museum has very little to do with the Linnæan test. Preserved animals are sent by

collectors to the taxonomist, who proceeds to classify them in new species, varieties or genera in the vast majority of cases without any experimental knowledge as to their breeding habits. Hence the terms species and variety are in practice used to a large extent interchangeably, though not deliberately. The historic problem of the origin of species is not that of the origin of museum species but of Linnæan species. If we can show that discrete hereditable properties arise, as we know that they do, discontinuously in the normal course of generation, we have all the materials we need to interpret the origin of varieties, genera, orders, families, classes, phyla. Whereas all these are arbitrary groups defined in terms of similarity and difference of the hereditable anatomical properties of animals and plants, the species, as defined by Linnæus, is a group limited not merely by the anatomical resemblance of its individual members but also by *their inability to breed successfully with other forms.*

What has been said so far about the origin of new hereditable properties bears directly upon the way in which new varieties arise. New varieties will only retain their characteristics if some external agency is employed to prevent them from hybridizing and thereby giving rise to an indefinite number of new combinations of characters. The Yokohama can be made to retain those characteristic differences which distinguish it from a White Leghorn by the mechanical device of separating the two strains with a partition of wire netting. No wire netting is required to prevent a White Leghorn and a turkey from losing their genetic individualities, when they are placed in propinquity to one another as are closely allied species in Nature. There is therefore in addition to the problem of

the origin of new varieties a problem of the origin of species incompatibility. This cannot be dismissed as of no importance, so long as our experimental knowledge of the origin of varieties fails to suggest in what way this incompatibility may arise. If we can solve this new problem the evolutionary hypothesis presents no ulterior difficulties in the way of explaining the origin of differences which separate the larger systematic groups. The differences employed in distinguishing genera from varieties, and orders from genera or classes from orders are differences of degree. Between species, superficially at least, there seems to be a difference in kind. On this account the origin of species has always been taken to signify the core of the evolutionary problem.

In a somewhat panegyric vein Mr H. G. Wells replying to Hilaire Belloc makes the following remark: "Darwin's book upon the subject was called *The Origin of Species*. It was a very modest and sufficient title. He did not even go to the length of calling it the origin of genera or orders or classes". Surely Darwin might much more appropriately have employed the latter. How types which are structurally different arise may or may not be accounted for by the selection hypothesis. How types which will not breed with one another arise within the same stock is not revelant to it. It is true that Darwin and Wallace vaguely referred in their writings to a natural tendency to infertility as forms become more sharply differentiated. This does not meet the difficulties of the case, even if it is a sound experimental doctrine. Bateson has used the following illustration to emphasize the irrelevance of Natural Selection to the species problem in the strict sense of the term:

"Sometimes specific difference (anatomical differences between species) is to be seen in a character which we can believe to be important in the struggle, but at least as often it is some little detail that we cannot but regard as trivial which suffices to differentiate the two species. Even when the diagnostic point is of such a nature that we can imagine it to make a serious difference in the economy, we are absolutely at a loss to explain why this feature should be necessary to species A, and unnecessary to species B, its nearest ally. The house sparrow (*Passer domesticus*) is in general structure very like the tree sparrow (*P. Montanus*) . . . They differ in small point of colour . . . The two species therefore, apart from any difference that we can suppose to be related to respective habits, are characterized by small fixed distinctions in colour-markings, by a striking difference in secondary sexual characters and by a difference in variability. In all these respects we can form no surmise as to any economic reason why the one species should be differentiated in one way and the other in another way, and I believe it is mere self-deception which suggests the hope that with fuller knowledge reasons of this nature would be discovered."

It is permissible to argue that the final justification of the evolutionary argument will be achieved when intersterile mutants have been shown to appear under experimental conditions. We shall then be able to state that new types which display not only anatomical but specific discontinuity have arisen in the ordinary course of generation. At present it is only possible to say that we have very good reason to believe they can do so. Bateson overemphasized the difficulty of the species problem when he said " the production of an indubitably sterile hybrid from completely fertile parents, which have arisen under critical observation from a common origin . . . is the event for which we wait ". Although the origin of the species barrier does introduce a novel issue into the discussion of the evolutionary problem, its novelty is not so fundamental as it appears to be at first sight. Morgan remarks with justice :

> "The necessity of putting the mutation theory to the test that Bateson calls for seems to me very doubtful, for while this is one of the possible ways in which a mutant might split off at once from the parent type, it is by no means the only way or even, I think, the most probable way in which species have become separated. . . . There is no one problem of infertility of species and no one problem of the sterility of hybrids, but many problems, each due to differences that have arisen in the germinal material. One or more of these differences may affect the mechanism of fertilization or the process of development, producing some incompatibility."

Bateson performed a most important task in emphasizing that the problem of species discontinuity exists. He made its solution assume more formidable proportions than the facts merit. There is no mysterious wholeness about the concept of the species barrier. Like other scientific concepts it defines a class of properties. When we examine the characteristics of species barriers, we at once see that they constitute a very heterogeneous assemblage of hereditable properties, many of which are recognizably similar to hereditable properties which we know to arise as mutants in genetic experiments. An individual may be placed in a different species from another individual because of some merely anatomical difference in the structures associated with the copulative act. Owing to the respective absence of neck hackles and tail feathers in two strains known as the Barbadoes and Rumpies, the male of the latter cannot successfully tread the female of the former, though each is interfertile with other breeds of domestic fowls. The origin of such differences does not constitute a problem of a different class from the origin of other varieties. High and low fertility are hereditable properties that can be studied as varieties within the species group. They have arisen as mutant characters in experiment. If there arose within a stock mutants with complementary genes for infertility

either type would be infertile with respect to the other. They would constitute separate species in the Linnæan sense, when the parent stock died out. In the case of the donkey and the horse, we can go further and identify the complementary sterility factors in the structure of the chromosomes. Difference of size and shape in the chromosomes of the donkey and horse prevent them from pairing in the reduction division, so that no ripe sperm is formed in the testis of the mule. Mutants differing with respect to chromosome numbers and sizes arising by fragmentation or fusion are known both in plants and animals to have arisen under experimental conditions. In many plants they have been perpetuated by self-fertilization. Plough has raised a mutant strain of Drosophila which is more fertile *inter se* than with the wild stock. The genetic basis of interspecific sterility, while worthy of much more extensive research, is now reaching a precision which places the experimental data of evolutionary theory beyond the plane of Malthusian speculation.

The foregoing illustrations do not exhaust the variety of biological characteristics which separate one individual from another as a member of a different Linnæan species. Nor do they exhaust the types which can be brought within the realm of experimental treatment. Other cases are discussed at length in Crew's *Animal Genetics*. The species barrier is not one thing but many things. In the light of modern research there is no reason to regard the origin of species barriers as an essentially different problem from the origin of varieties. Nevertheless the two issues are superficially distinct. No discussion of the present status of the evolutionary hypothesis is complete unless the distinction is submitted to critical examination in the light of experiment.

In the opening years of the twentieth century it had become the fashion among biologists to treat evolution as a dogma. The growth of experimental study of inheritance and variation tends rather to make us value it as a hypothesis suggestive of further enquiry. The difference between the two attitudes is akin to a difference of method which mankind has adopted throughout the ages in the pursuit of knowledge. One method rationalized in its most rigid form in the philosophy of Hegel is to seek for some proposition to which every one is agreed and proceed by deduction to whatever conclusions may be reached from the starting-point. This method has proved invaluable to politicians and members of the legal profession in the discharge of their vocational activities. It is essentially like that of the schoolmen who would exhaust themselves in untiring search into the writings of the ancients for some authoritative statement regarding the number of teeth which the horse possesses, a statement that no one would dare to question. The scientific method is irreconcilably opposed to the Hegelian method. With no aspirations to good breeding it prefers to look the gift horse in the mouth. It is just those propositions which every one accepts that the scientist is most anxious to examine in the hard light of experience. In attempting to envisage a natural mechanism by which the graded differentiation of animal structure could have been brought about, Lamarck was content to employ the generally accepted belief in the inheritance of acquired characters without bringing it to experimental test. Darwin, fortified with newer knowledge of the historical succession of animals and plants as recorded in the rocks, sought to show that evolution was a necessary consequence of competition and the " strong principle of inheritance ".

Darwin did not undertake the task of enquiring into the nature of the "strong principle of inheritance". It was to him like one of Euclid's axioms. Mendel alone at this time saw the necessity for an experimental study of inheritance, and pointed the way to a non-dialectical treatment of the problem.

VII

NATURAL SELECTION AND EXPERIMENTAL RESEARCH

"Heredity as something quite incomprehensible cannot be used as an explanation, but only as a designation for the identification of a problem. And the same holds good of adaptability."—Nietzsche, *The Will to Power*

§ 1

To large numbers of people evolution is Darwinism, just as to our fathers geometry was Euclid. In one of his writings Morgan has remarked that "it is not so important to find out whether Darwin's ideas were as clear as our own, as to make sure that our own ideas are clear". This is true; but an interest in the history of scientific thought is a blameless pursuit for its own sake; and there are ulterior reasons which justify an historical discussion of the criticisms which experimental discovery has brought to bear on the Selection doctrine in its original form. During the latter half of the nineteenth century the evolutionary hypothesis became entangled with the idea of a moral progress of mankind. On this account some philosophers, who are not biologists themselves, fail to recognize the ethical neutrality of biological enquiry. It is doubtful whether the promulgation of any scientific hypothesis has ever had so profound and, at the same time, so immediate an effect on the attitude of educated people towards personal responsibility and social obligations. The fate of Darwinism is as much the concern of the layman as of academic biologists.

Nor is it easy for those who are not biologists to gain definite enlightenment concerning the extent of the change

that has taken place. With the rise of experimental method the discussion of evolution has become more technical owing to the accumulation of new data and on account of the introduction of a more intricate form of logic. It is a quantitative branch of science. There was a time when the biologist thought it worth his while to read and to reply to Samuel Butler. To-day there are biologists who read—and like the present writer enjoy—the works of Mr Bernard Shaw. They do not feel it necessary to defend their philosophy against the arguments advanced in the preface to *Back to Methuselah*. Popular expositions of evolution are still written. More often than not one suspects that they are rather too popular to answer the questions which an intelligent reader who is not a biologist is most anxious to hear discussed.

As an exact science biology is still very young. Evolution is in its infancy. Only in our generation has it become the nucleus of a growing body of experimental research. It may be that when the history of the evolutionary hypothesis is written two centuries hence, Bateson's *Materials for the Study of Variation* will assume a more prominent place than *The Origin of Species*. It may be that the name of Thomas Hunt Morgan will be mentioned in its pages more often than that of Charles Darwin. We are too near the footlights to view the matter in its correct historical perspective. It is at least permissible to entertain such a possibility. Ancestor worship has no place in the ritual of science. If any display of sentiment is appropriate in scientific discussion, it might be said that the only fit way in which to honour the memory of a Darwin and a Newton is to press forward in exploring the fields which their labours have fertilized.

Without entering into technicalities I shall make the attempt in this essay to contrast the use of the term Natural Selection in Morgan's writings with the Darwinian doctrine in its original form. My aim will be neither to justify in the one case nor exculpate in the other, but to discover whether a difference exists, wherein the difference lies, and how the difference has arisen. In contrasting the views held by two men of science it is of the utmost importance to lay emphasis on the type of data which they have respectively studied most. Morgan is an experimental geneticist. Darwin was pre-eminently a geographical naturalist. Morgan's most brilliant contributions to the advance of science have been focused on the study of those conditions which are significant to the origin and transmission of new hereditable properties in animals. Before the publication of *The Origin of Species* Darwin's scientific labours had concentrated more especially on amassing a wealth of information about the way in which species are distributed in different parts of the world. In his long itineraries, it is not difficult to surmise what aspect of the species problem was constantly uppermost in Darwin's thought. I think it is necessary to appreciate this bias in any attempt to understand the way in which the Selection hypothesis developed. Though Darwin spoke of the Origin of Species, he was interested primarily in why some species happen to be found in one place and other species in different places. Darwin had two distinct problems in view when he set out to write *The Origin of Species*. In the course of writing it he sometimes lost sight of the distinction between them. One was how different types of animals have come to persist in different parts of the world. The other was how an evolutionary process could take place at all. That the struggle for existence is

the key to the former is highly plausible. No facts are known which contradict such a view. It is not really an issue with which the modern experimentalist concerns himself. Up to this point there is no divergence between the Darwinian and the Mendelian standpoint. But Darwin in very unequivocal language committed himself to the view that in building up new specific forms the struggle for existence makes use of all differences between parent and offspring of "whatsoever" origin. He thus implicitly encouraged the view that natural selection is a creative agency. Herein lies a fundamental difference between the standpoint adopted by Morgan and the Darwinian doctrine. Darwin really believed in the Origin of Species by natural selection. Morgan believes in the Origin of Gaps by natural selection.

It is perfectly true that Darwin did not formulate this deduction so explicitly or so prominently as did some of his followers. But it was logically implicit in his earlier writings and very definitely set forth in his later. It was in virtue of this aspect of the Natural Selection hypothesis that evolution captured the support of Darwin's contemporaries. Till Darwin's book appeared, biologists did not for the most part believe that evolution could take place. Darwin's hypothesis demonstrated that evolution must take place in a world in which organisms had to struggle for their existence. The experimental data which Morgan employs as the basis for his conception of the evolutionary process imply that the reasons which led the pre-Darwinian biologist to think that evolution could not take place are unfounded. They also imply that the reasons which Darwin advanced to show that evolution *must* take place are wrong.

It is easier to make this distinction clear at a later stage

with the aid of a concrete example than by stating general propositions. This is because one result of experimental progress has been a change in our use of the concept of "variation". Darwin used the term variation for any difference between parent and offspring. In affirming that the struggle for existence makes use of all variations for building up species differences, he logically implied one of two things. Either all differences between parents and offspring are genetic in origin, that is to say, referable to differences in the egg or sperm; or alternatively bodily modifications which occur during the lifetime of an individual influence the genetic structure of the offspring so as to produce an analogous result. This principle, usually associated with the name of Lamarck, was accepted by every one in Darwin's time. Darwin himself, while ridiculing Lamarck's idea of the *modus operandi* of evolution, accepted the inheritance of acquired characters. There was therefore no need for him to make a distinction between the two alternatives. Neither the one nor the other is in harmony with the standpoint of a modern geneticist of Morgan's school; but the difference between the Darwinian standpoint and that of Morgan concerns not only the question of fact but the deductions drawn from it.

The difference between either of these alternatives on the one hand and the Mendelian standpoint on the other can be illustrated by reference to one of Mendel's original experiments on the hybridization of peas. In crossing pure-bred peas of the variety characterized by a dwarf shoot with the normal tall variety, Mendel obtained only tall types on the first generation, and in the second generation derived from crossing the latter *inter se* one-quarter were dwarf and the remaining three-quarters tall. Now the individuals

of either the tall or the dwarf class are not all alike. Any dwarf shoot grown under ordinary conditions is distinctly smaller than a tall shoot, so that the two classes are discontinuous and quite easily distinguishable; but when the conditions are standardized as much as possible small differences of light, moisture, soil-content, temperature or proximity exert their influence, so that no two dwarf plants are of exactly the same size. What is transmitted through the gametes is something which determines the extent to which an individual is capable of growing under appropriate conditions. This distinction greatly clarifies our thought about the so-called inheritance of acquired characters.

A criticism of the Lamarckian doctrine is irrelevant at this juncture. It is referred to in this connexion because it was only in the eighties, after the Lamarckian view was challenged by Weismann, that the full force of the logical implications of Darwin's teaching was felt. It is true that his followers were far more definite than the author of *The Origin of Species* in emphasizing the creative rôle of selection. It is true that the discredit into which the Lamarckian principle fell after the discovery of the nature of fertilization led the Selectionist writers to exaggerate this aspect of Darwin's hypothesis. Nevertheless Darwin did express himself in unmistakable language with regard to this issue. His followers, forced to be more specific concerning the nature of differences between parents and offspring, made the bold, and, it transpired, unwarranted assumption that all those small differences between parent and offspring now referred to as fluctuating variability are in the main genetic in origin. The Selectionist doctrine thus assumed that hard outline which produced its first vigorous reaction in

Bateson's *Materials for the Study of Variation* (1894), a work which laid down the main lines of investigation which have been elucidated by the Mendelian renaissance.

To avoid vagueness concerning what Darwin actually did say I shall quote once more from *The Origin of Species:*

> "Any being, if it vary in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be naturally selected. From the strong principle of inheritance, any selected variety will tend to propagate its new and modified form." (Introduction.)
>
> "Each of the endless variations which we see in the plumage of fowls must have had some efficient cause; and if the same cause were to act uniformly during a long series of generations on many individuals, all probably would be modified in the same manner" (Chap. 1.)
>
> "A high degree of variability is obviously favourable as giving the materials for selection to work upon, not that mere individual differences are not amply sufficient, with extreme care, to allow of the accumulation of a large amount of modification in almost any desired direction." (Chap. 1.)
>
> "Over all these *causes* of change, the accumulative action of selection, whether applied methodically and quickly, or unconsciously and slowly but more efficiently, seems to have been the predominant power." (Chap. 1.)
>
> "Variations, *however slight, and from whatever cause* proceeding, if they be in any degree profitable to the individuals of a species, in their infinitely complex relations to the individuals of a species . . . will tend to the preservation of such individuals and will generally be inherited by the offspring. The offspring also will have a better chance of surviving, for of the many individuals of a species which are periodically born, but a small number can survive. *I have called this principle, by which each slight variation if useful is preserved, by the term Natural Selection.*" (Chap. 3.) (Italics inserted.)

If, as Darwin believed, it were true, that variation occurs in every generation, the evolutionary process would be a continuous one. To Morgan the production of mutants is a

discontinuous break in a normal routine of stability. To Darwin variation and heredity were co-extensive terms. The offspring are always on the whole like their parents. That resemblance constitutes inheritance. On the other hand they are never quite the same. The difference was what Darwin called variation. To Morgan heredity and variation are not co-extensive terms. The structure of the chromosomes is fundamentally stable. From time to time there occur disturbances of this normally stable equilibrium. New hereditable properties emerge into being in a quite discontinuous fashion. There is no self-evident reason why a particular stock should not remain indefinitely in a phase of stability. To the experimental geneticist there thus exists no difficulty in interpreting the fact that some animals have remained unchanged since the earliest rocks.

To the generation in which Darwin lived there seemed to be only one logical outcome of the view that variation is a continuous process involving all the individuals of every generation. This deduction was never stated very explicitly by Darwin himself, though it was definitely asserted by Wallace. There can be no doubt that this deduction gave the Selection hypothesis such a strong appeal to Darwin's contemporaries, and contributed largely to the success of the hypothesis of Natural Selection. Before Mendel, investigators in hybridization had treated the individual as the unit for study. From this arose the belief that hybrids are intermediate between the parents. This belief in its turn gave rise to the notion that on crossing a new type back to the parent stock there would be a dilution of the new character, culminating after a number of generations in swamping it out of existence altogether. Evolutionists of the Darwinian period therefore introduced

a variety of devices, such as geographical isolation and, above all, the survival of the fittest, to counteract the effect of this swamping and account for the persistence of new types. To Darwin's generation it seemed that without selection there could be no evolution. The new type would always be swamped out in the long run. In the struggle for existence the less viable variations would tend to be eliminated, and since there would always be less of them on that account, the swamping process would favour the gradual moulding of the species in the direction of more favourable variation. On this view the struggle for existence is the agency which makes species change. Evolution becomes a necessity.

From Morgan's standpoint evolution is only a necessity in so far as it happens that mutants do from time to time appear. The struggle for existence though eliminating the less viable types has no creative rôle in the Darwinian sense. Mendelian analysis shows that though the first generation of a cross between pure-bred parents may be intermediate between the parental types, both parental types appear in their original purity in the next generation, and will continue to breed true to type, whenever they mate with other individuals similarly constituted. The modern geneticist feels no necessity for an *argumentum ad hominem* to explain how evolution can occur in spite of a supposed swamping process. To him the swamping process is an illusion based on imperfect knowledge of the facts of hereditary transmission. The importance of this difference in standpoint lies in the fact that the idea of natural selection would never have assumed so powerful an influence over biological thought, unless it had provided the evolutionist with train of reasoning which seemed to prove that evolution must be going on all the time.

This interpretation of the Darwinian standpoint is not a caricature drawn by the pen of an adverse critic. An enthusiastic contemporary exponent of Natural Selection, Mr H. G. Wells, thus defines the selection theory in his *Outline of History*:

> "the young which a living thing produces . . . are like the parent living thing. But they are *never exactly like it* or like each other. . . . Suppose, for example, there is some little furry whitey-brown animal living in a bitterly cold land which is usually under snow. Such individuals as have the thickest, whitest fur will be least hurt by the cold, less seen by their enemies and less conspicuous as they seek their prey. The fur of this species will thicken and its whiteness increase *with every generation*, until there is no advantage in carrying any more fur." (Italics inserted.)

Having cited the above, it is somewhat surprising to note that in replying to Mr Belloc's strictures, Mr Wells makes the following statement with reference to the Natural Selection theory:

> "Among questions bearing upon it but not directly attacking it has been the discussion of the individual difference. . . . What rôle is played by what one might call normal relatively slight differences and what by the sports. Can differences establish themselves while outer necessity remains natural? Can variations amounting to specific differences . . . be tolerated rather than selected by Nature? . . . What happens to differences in cases of hybridization? . . . None of these subsidiary questions affect the stability of this main generalization of biology."

In explaining the Natural Selection theory, as quoted above, Mr Wells himself states or implies every one of these "subsidiary" questions, and answers them in his own way.

Let us now see how a modern geneticist would interpret the evolutionary process by taking an analogous concrete example. He would argue somewhat as follows. Supposing a single white mutant hare arises in a grey parent stock,

the behaviour of the chromosomes leads us to infer that eventually other white hares, pure for the white gene or genes, will reappear. These mated *inter se* will breed true to type. On the assumption (not conclusively proved) that it is advantageous for a hare in temperate climates to be grey and in arctic regions to be white, there will be more white hares in the long run in northern countries and more grey ones in temperate countries. If there were no competitive struggle at all, there would in the long run be grey and white hares in northern and grey and white hares in temperate countries. There would have been the same amount of evolution. The only difference that the struggle for existence introduces is that the final picture presents a more discontinuous aspect. This was not at all what Darwin meant by Natural Selection. He would have said that a single mutant would be swamped out of existence by intercrossing. He would have formulated the problem in the following terms. Of all hares born to grey parents some are lighter and others darker. In a region where it is advantageous, the half that are lighter than the mean will have more chance of surviving to maturity. In any given generation there will therefore be more lighter than darker parents. The result of this will be that in every generation the swamping process will always be on the side of the lighter individuals. Darwin postulated that, if this process went on long enough, a white hare would eventually be produced. Such a race would only be produced in the region where natural selection favoured its survival. On this view natural selection is the creative agency, or at least a paramount creative agency in the evolutionary process. Without the struggle for existence hares everywhere would remain grey. In every generation the half

that are lighter than their parents would always be swamped by the half that are darker.

To Darwin and more especially to Darwin's followers selection was the agency which preserved not merely new individuals but new characters, since characters would otherwise be diluted out of existence. For Morgan the preservation of new characters ultimately resides in Mendel's law of segregation. It has its material basis in the behaviour of the chromosomes. The contrast between the alternatives is at once made clear when we consider what would happen in a universe so large and so abundantly supplied with the necessities of life that no struggle for existence intervenes. Given unlimited time in a Mendelian universe in which natural selection did not operate, all the species we know to-day would be present, and many more besides. Evolution would have occurred; but the pageant of life would present to the taxonomist a more continuous appearance, and the striking gaps which we now see would be filled not by fossil relics but by living forms. Except in so far as he was prepared to invoke the Lamarckian principle to circumvent difficulties inherent in his own hypothesis, natural selection was to Darwin the necessary condition not merely for gaps but for any evolution to take place at all. In a Darwinian universe without natural selection there would be no progressive differentiation of new characters.

§ 2

When, out of deference to Darwin's contribution to biological thought, the experimentalist of Morgan's school asserts his belief in Natural Selection, he is in fact referring to something very different from Darwin's Natural Selection,

indeed to a view of the process which Darwin would have rejected emphatically. Of course it is admitted that all scientific hypotheses become modified as new data accumulate; and phrases imperceptibly change their meaning in the course of time. But the natural selection of Morgan's school is not a continuous development from the original concept. Within two decades of the publication of *The Origin of Species* the selection hypothesis had assumed a clarity of outline which had an influence on subsequent developments in biological thought, persisting till the present day, and not likely to disappear for some time. In 1881 Weismann challenged the prevailing belief in the inheritance of acquired characters. Thenceforth in the hands of the Selectionists environment became merely an agency by which the hereditary materials are preserved or rejected. As an aspect of the problem of development it faded into the background of the picture. To question the almightiness of heredity became equivalent to defending the Lamarckian principle, though the two issues are logically independent.

Educated people frequently use the words environment and heredity in a very different sense from that in which they are employed by the biologist. Unless we are accustomed to the study of embryonic and larval life, we are apt to think of an organism as a finished product. The rôle of environment and of heredity as seen through the eyes of a contemporary biologist can be made explicit by reference to recent work on the metamorphosis of tadpoles. We know to-day that the thyroid gland of all vertebrates contains a high percentage of iodine. Barger and Harrington have now prepared in pure crystalline form an iodine compound which has the same therapeutic properties as extracts of the thyroid gland. A few years ago the discovery that

frog tadpoles will change very rapidly into adults if fed with thyroid gland, was followed up by the development of a successful technique for removing the rudiment of the thyroid gland in frog embryos. Thyroidless tadpoles never undergo metamorphosis. They continue to grow as tadpoles when the normal tadpole would change into a frog. The change into the adult in the normal tadpole is initiated by the liberation of the thyroid secretion into the circulation, It has also been shown that tadpoles reared on an iodine-free diet in water containing no trace of iodine remain permanently in the larval state. This clarifies what is meant by an *environmental* factor in development. In contradistinction to the influence of environment the influence of inheritance in development may be illustrated by reference to an American salamander, *Amblystoma tigrinum*, which has a characteristic larval form. In the lakes around Mexico city there is a local race of this species which never undergoes metamorphosis in nature, reproducing in the larval form. It can be made to develop into the land-dwelling adult in a few weeks, if fed with thyroid gland in the laboratory. Addition of iodine salts to the water in which it lives or to its food will not induce metamorphosis. Its permanent fixation in the larval stage is due to the fact that it *inherits* from one generation to another a deficient thyroid gland, which cannot make use of the iodine in its surroundings. Absence of iodine in minute quantities from the water, a purely environmental agency, or on the other hand a hereditary difference between two races with respect to the efficiency of thyroid secretion, may either of them be *independently* instrumental in deciding whether a particular individual shall attain sexual maturity in the form of an air-breathing land-dwelling salamander, or an aquatic half-way house between a salamander and a fish. A geological epoch, if you like to put

it in that way, is thus summed up in a mutant gene or in a trace of iodine.

In the attempt to understand the tenacity with which belief in the Lamarckian view persisted in biological thought, it must be borne in mind that embryology is the most recently developed branch of anatomical science. Until the classical researches of von Baer and Meckel were published in the first half of the nineteenth century, the prevailing idea about development was the teleological doctrine that an animal is from the very first complete in all its parts and only needs growth to make its minute structure manifest to the eye. Caspar Wolff in 1759 made observations on the hen's egg, and was led to state the "epigenetic" as opposed to the prevailing "evolutionary" view. He sought to show that the hen's egg is at the beginning without any gross anatomical organization and that structural organization within the egg is a gradual development. His work failed to attract attention. Von Baer's researches on the same subject were published synchronously with the formulation of the Cell doctrine (1832). One might say that until the middle of the nineteenth century, the current conception of inheritance in biology was closely analogous to the legal notion. The parent was supposed to hand on its anatomy to its offspring in the same sense as the well-to-do hand on their belongings. With so erroneous a conception of the nature of development prevailing, it is little wonder that the idea of the inheritance of acquired characters seemed a perfectly reasonable one. It is not surprising that the doctrine of Lamarck should have been first challenged during the decade in which the nature of fertilization and the process of maturation of the germ cells were elucidated.

As stated by its author the Lamarckian principle implied that any reaction of the organism to its environment is carried over to subsequent generations. It was especially *adaptive* reactions such as the effect of use and disuse which Lamarck emphasized in his evolutionary speculations. When the Lamarckian principle was first challenged, prominent scientists like Cope were willing to assert such fables as the story that a cock deprived of one eye transmitted eye defects to all his offspring. When it was conclusively proved that mutilations effected through several generations left no impress on the hereditable characters of the stock, the Lamarckians fell back on the gratuitous postulate that only " adaptive " changes could be transmitted. The precise meaning of this adjective was never defined, nor was any reason forthcoming to suggest the existence of a mechanism that could discriminate between mutilations and bodily changes that are " adaptive ". This is yet another example of the perils of introducing teleological preoccupations into the construction of biological hypotheses. If recent experimental research conserves any element of truth in the Lamarckian idea, it has robbed it of any special significance to the way in which adaptive structures originate.

Structural changes may arise in the course of development from two conceivable sources. The chromosomes which represent the hereditary materials may find themselves reacting to a different type of " internal environment ". The majority of modifications in the normal course of development undoubtedly come within this category. Modifications of this type, including in all probability relative sizes of organs, all mutilations and habits are clearly not hereditable. Belief in their hereditability was only possible so long as biology was dominated by teleology

and the essential features of the reproductive cycle were undiscovered. There is another possibility which was entirely disregarded by Weismann in his Theory of the Germ Plasm. It is a possibility that has no bearing on the problem of adaptation. If environmental agencies can produce mutations by a structural change in the chromosome itself, there is no reason why such structural changes should be confined to the chromosomes of the germ cells. We must therefore preserve an open mind with regard to the possibility of encountering phenomena having a superficial similarity to what is implied in Lamarck's doctrine. The exposure of young larvæ of the fruit-fly to X-rays has led to the production of indviduals which show bodily resemblances to forms which have arisen in the ordinary course of events as mutants. The effect of X-rays may be to change the environment in which the chromosomes operate. But the recent investigations of Patterson indicate the likelihood that the modification is due to the action of the X-rays on the chromosome itself. We know that X-rays will produce mutant changes in the chromosomes of the germ cells. If Patterson's interpretation is correct, it may well be found that X-rays can simultaneously effect mutant changes in all the chromosomes of the body. If applied sufficiently early in the course of development, radiation with X-rays would then produce bodily changes of a transmissible nature. This possibility resides in the fact that the agent is capable of acting on all the cells of the body in the same way at the same time. There is no inherent unlikelihood that temperature and the chemical constituents of an animal's food may simultaneously produce bodily and germinal mutations. Strictly speaking this is not the same as the traditional belief in the "inheritance of acquired

characters". The Lamarckian principle completely disregards the distinction between modifications which arise from a change in the internal environment of the chromosomes and a physical change in the chromosomes themselves. It takes no account of the possibility that the environmental agent can act in the same way simultaneously on all the cells of the body.

There are still students of fossil forms who claim that the traditional Lamarckian view is necessary to explain the historic succession of animals by continuous generation. There seems to be no satisfactory reason to justify the statement that evolution can only be satisfactorily explained by assuming the inheritance of acquired characters. If there were, it would not be an argument in favour of the Lamarckian principle. It would be as an argument against the evolution theory. It would imply that the truth of evolution depends on assuming a mechanism whose existence is most unlikely. What is often called the neo-Lamarckian standpoint, the view that acquired characters only gradually become impressed on the hereditary constitution after countless generations, transfers the issue from the plane of verifiable experience to one of pure surmise, rendering further discussion profitless. In such a matter as this when experiment is silent, the student of fossils must also be silent.

The objection rests in fact on a misapprehension. The earlier phase of experimental enquiry along the lines laid down by Mendel was confined to the analysis of simple clear-cut hereditary differences which present themselves in almost any environment in which the animal can live. They were also largely concerned with differences that could be resolved into the simplest arithmetical ratios, or as

Morgan would say with mutants that have arisen through a change at a single point on one pair of chromosomes. It is only as technique has progressed that it has been possible to analyse the more complex cases in which single characteristics depend on numerous Mendelian factors, or where the character differences are so variable that they can only be defined in statistical terms. The palæontologist being occupied very largely with size differences is sometimes disappointed, because such phenomena lie outside the scope of the simpler problems, which were once thought to define the scope of the Mendelian hypothesis. Recent progress which has led to the recognition that Mendel's principle of segregation underlies the inheritance of size is therefore of no little significance to evolutionary theory. As we come to recognize the dependence of hereditary transmission on discrete particles which maintain their entities uncontaminated through all the cell divisions of the body, segregating in their entirety in the formation of the gametes, the unlikelihood of the Lamarckian principle in its traditional form becomes more and more evident.

If the Lamarckian principle in its traditional form was undoubtedly based on a confusion of ideas and an ignorance of fact, the Theory of the Germ Plasm put forward by Weismann shows how facts may be distorted to fit in with preconceived ideas which are in themselves logically flawless. The discredit into which the Lamarckian principle fell, almost as soon as the elementary facts about the nature of fertilization became known, led Darwin's successors to assume that all those differences between parent and offspring which Darwin had referred to under the term variations are genetic in origin. The assumption was gratuitous, as later experimental analysis has shown. Without that

assumption the Selection doctrine would have been robbed of the immense importance it had already begun to assume. From a complete misapprehension of the true rôle of the environment in relation to inheritance, the biological pendulum swung in the opposite direction to a complete disregard of the influence of the environment in relation to development. It is from Weismann's writings that we can best appreciate the fundamental dissimilarity of Darwin's Natural Selection and Morgan's views. For Weismann's "germinal selection" is the logical outcome of Darwin's selectionism, once it had been purged of the Lamarckian principle. It is a triumph of Hegelian reasoning applied to biology. There is nothing wrong with it but its premises. Weismann's theory embodied an atomistic conception of heredity. Unlike Mendel's it had no connexion with experimental data. Weismann identified his hereditary determinants with the substance of the chromosomes. Unlike Morgan's hypothesis, Weismann's speculations were based on incorrect observations about the way in which the chromosomes behave. In the long run the influence of Weismann's teaching has probably been more sterilizing than the Lamarckian doctrine which he challenged.

Weismann imagined that his atoms of heredity or "determinants" multiply in the cell and in some rather abstract way compete with one another for survival. Hence the hereditary constitution of the individual is never quite the same in two successive generations. Heredity and variation are thus co-extensive, as Darwin's Natural Selection postulates. Weismann also thought wrongly, it transpired, that the reduction division of the germ cells takes place in such a way that each cell receives half a maternal and half a paternal chromosome of each pair and not, as we now

know, a whole paternal or a whole maternal element. Hence he argued that the formation of the germ cells involves not, as Mendel proved by experiment, a segregation but a closer intermingling of the germinal materials. From this the swamping of new characters on crossing became an absolute necessity. To Weismann selection alone could prevent this swamping. Selection must act in every generation, because the mingling of the hereditary materials becomes more intimate with every generation. Only under the influence of continuous selection could any change be brought about. Without it universal stagnation would exist. In short Selection was the creator and the preserver of the benefits of variation. In all this Weismann, with the support of Wallace, went much further than Darwin himself. But the Selectionist doctrine in its main features was implicit in the Origin of Species. The sociological exploits of biologists belong especially to the period in which the Selection doctrine assumed this doctrinaire aspect. Doctrinaire Selectionism has persisted in our own generation in the writings of many eugenists.

§ 3

We set out in the first place to contrast the views of the modern geneticist with the Selection hypothesis in its original form. The main differences arise in connexion with two issues. One concerns Darwin's own view that evolution is a continuous process. Darwin believed that selection operates on all the individuals of every generation. This implies either that acquired characters are inherited or alternatively that all differences between parent and offspring are hereditary differences in the modern sense. The views to which modern geneticists have been led by their

experiments are diametrically opposed to both conclusions. The other question concerns the creative rôle of selection. This belief arose from ideas about hybridization and artificial selection current among those biologists to whom Darwin addressed his argument. Darwin himself did not stress the point; but it was this corollary of his theory which accounts for the successful appeal which Natural Selection made to Darwin's contemporaries. They were satisfied that, if a struggle for existence occurs, evolution must be taking place. This was because all biologists before Mendel confused the characters which do blend with the genes that do not. To the modern geneticist this corollary has no significance, because experiment has forced him to reject views about hybridization prevalent before the publication of Mendel's researches. To Morgan, as to Darwin, selection through the survival of the fitter is essentially like artificial selection. Morgan differs radically from Darwin in his understanding of the way in which artificial selection itself operates. According to Morgan selection has no creative significance. "Selection has not produced anything new, but only more of certain kinds of individuals; Evolution however means producing new things, not more of what already exists."

Thus from the standpoint of Morgan the status of evolution is more satisfactory in the light of modern research. For there is no need to advance any special device to explain why new types are not swamped out of existence through the blending of characters on crossing. From the point of view of the Darwinians, if they were still with us, the outlook would be disconcerting. The modern geneticist no longer regards evolution as an imperative consequence of the struggle for existence. On the other hand the modern

view presents no greater difficulty than the former one in explaining the tendency towards greater adaptation. It is free from the objection that it proves too much. New hereditary types would persist even if there were no struggle for existence. Since there is one, the chance that a given mutant will reach the age at which it can produce offspring will be greater if the mutant character has " survival value ". At present there are insufficient experimental data to make profitable the discussion of the amount of advantage necessary to ensure survival. At the same time it is of interest to record that the application of Mendelian method furnishes materials for a precise statement of what selection can achieve and the rate at which it works, when the extent of differential fertility or mortality in a population is known. The mathematical theory of selection has been made the subject of some illuminating researches by J. B. S. Haldane and by Fisher. Haldane's calculations have led him to conclusions very different from the dialectical deductions which some eugenists have drawn from the recent decline of the European birth rate.[1]

[1] Two quotations from his writings may be added to justify the foregoing criticism of Weismann. Concerning the essentially creative rôle of Natural Selection he wrote, " The transformation of a species as well as the preservation of its constancy are based upon natural selection, and this is incessantly at work, never ceasing for a moment." (*Germ Plasm*, p. 414.)

Elsewhere he states that heredity and variation are coextensive. " We have seen that this transmission affects the whole organism, and extends to the most trifling details, and we also know that it is never complete, and that the offspring and parent are never identical, but that the former always differs more or less from the latter. These differences give rise to the phenomenon of *variation, which thus forms an integral part of heredity, for the latter always includes the former.* (*The Germ Plasm*, p. 410.)

VIII

THE SURVIVAL OF THE EUGENIST

"I am that ancient hunter of the plains,
That raked the shaggy flitches of the bison:
Pass, world: I am the dreamer that remains,
The Man, clear-cut against the last horizon."

Roy Campbell, *Flaming Terrapin*

CONCERNING Vesalius one of his biographers has said: "in dissecting monkeys he became convinced that the many discrepancies between the Galenic teaching and his own observations on the human body were due to the circumstance that Galen had derived most of his knowledge from dissecting monkeys, and had not thought it necessary to mention the fact". Perhaps the biographer of a future Vesalius who succeeds in laying the foundations of social anatomy will record that "in studying the writings of the Eugenists he became strengthened in the conclusion that they were discussing the habits of fruit flies rather than human beings, but had not thought it necessary to mention the fact".

I have called this essay *The Survival of the Eugenist*; but I wish to make it clear that I entertain no lack of sympathy for *Eugenics* as defined in general terms by Galton, the Galen of social biology. I have chosen this title to lay emphasis on the part which eugenists have played in perpetuating a certain attitude towards human society. This attitude starts from an examination of those characteristics which man shares with all other animals, but neglects the equally important task of defining those characteristics which distinguish man from all other animals. The weak-

ness of all mechanistic systems hitherto proposed lies in their refusal to recognize the existence of anything which does not yet come within the province of scientific method. A mechanistic philosopher can legitimately entertain the hope that the study of human society will become an ethically neutral science, and that the methods of biology will fertilize sociological enquiry, as the methods of physics and chemistry have fertilized biological investigation. He is not entitled to pretend that biology can at present provide a key to the interpretation of human history. I am well aware that there are eugenists who would repudiate any such pretensions. At the same time the general tendency of eugenic propaganda has been to exaggerate, and grossly exaggerate, the applicability of genetic principles to the analysis of human society. This tendency is a legacy of the period in which Eugenic ideas had their origin.

Whatever disadvantages the Christian cosmogony imposed upon the study of human society, it possessed the merit of emphasizing that the proper study of mankind is man. The immediate influence of the evolutionary controversy was a reversion to the Galenic practice in social anatomy. There is nothing surprising in this reaction. To Huxley and Spencer the important fact was that Man is a brute. It was necessary for them to emphasize man's genetic similarity to other animals in opposition to the traditional view which placed man in a special category apart from other natural objects. How strongly the need to emphasize Man's new status was felt can be inferred by a well-known dictum in *Man's Place in Nature*. "Whatever systems of organs be studied", wrote Huxley, "the comparison of their modification in the ape series leads to one and the same result—

that the structural differences which separate man from the gorilla and the chimpanzee are not so great as those which separate the gorilla from the lower apes ". In his dispute with Owen, Huxley went much further than any modern anatomist would be prepared to follow him. If like Cuvier he had based his objections on the structure of the human foot instead of the hippocampus major, Owen might have made a stronger case. His opponents were too busy disposing of man's Cartesian spirit to devote much attention to his Cuvierian sole.

The evolution of Thomas Henry Huxley, of Herbert Spencer and of Francis Galton was a precocious baby. Its parents and relatives entertained high hopes of its future career. In that tradition it has been nursed by their loyal disciples who have encouraged it to discourse upon sociology before it has learned to read and write. Huxley, Spencer and Galton were fundamentally right in recognizing that any theory of the development of human society implies certain biological assumptions. Their anticipations of immediate progress in the biological treatment of human society was inevitably coloured by the issues which made the first claim on their attention. Those issues are no longer topical. The experimental biologist of to-day cannot approach the structure of human society from quite the same angle. The pioneers of evolution were goaded by theological opposition to adopt an attitude which is easy to condone but unnecessary to emulate. To justify their right to speculate, they found it necessary to convince the non-scientific public that their speculations were correct. To do so they were driven to minimize the gap between man and the apes and make the best of any evidence pointing to the missing link which popular imagination demanded.

The missing link provided the occasion for one of the first sociological exploits of anatomical science. There is an account of the incident given in Dr. Haddon's *History of Anthropology*. Three years after *The Origin of Species* was published Dr. James Hunt, President of the Anthropological Society, read his paper on "The Negro's Place in Nature". In it he maintained that "the analogies are far more numerous between the ape and the negro than between the ape and the European". In 1866 he recorded a further contribution to the detection of the missing link by asserting that "there is as good reason for classifying the negro as a distinct species from the European as there is for making the ass a distinct species from the zebra". In this discussion Huxley gave the exponents of the missing link a half-hearted support tempered somewhat by his humane and sceptical disposition. An obituary notice of Dr. Hunt in a New York paper announced in 1870 the "Death of the Best Man in England". Sixty years after the publication of Hunt's first communication, a leading American anthropologist, Professor Kroeber, summed up the present state of knowledge in the following terms :

> "The only way in which a decision could be arrived at along this line of consideration would be to count all features to see whether the Negro or the Caucasian was the most unape-like in the plurality of cases. It is possible that in such a reckoning the Caucasian would emerge with a lead. But it is even more clear that which ever way the majority fell, it would be a well-divided count."

Speculation upon the ancestry of man has continued with unabated vigour to the present time. Huxley's generation had one good excuse for confusing the process of social and organic evolution. It cannot be pleaded by our own.

Modern men were known to be associated with the later palæolithic cultures. The Mousterian artefacts had been associated with the Neanderthal type. There was much to encourage the hope that further research would reveal a close parallelism between the physical differentiation of specific or racial types and successive stages of cultural development. It now appears that Mousterian artefacts were also fashioned by types who, as Sir Arthur Keith puts it, " would excite no comment, if dressed in modern garb in any assemblage of modern Europeans ". Our own species has served a long apprenticeship in a much earlier phase of cultural development than that which was at one time attributed specifically to the Neanderthalers. The data presented in Sir Arthur Keith's book *The Antiquity of Man* show that it is not easy to press blood relationships out of stone implements. There are already signs of a reaction against the extravagant claims which have been put forward by some physical anthropologists. The most recent hypothesis of the origin of civilization completely breaks with the earlier tradition to harp on the racial aspect of the problem. Professor Elliott Smith is distinguished both as a physical and cultural anthropologist, and it is therefore noteworthy that his theory emphasizes the characteristics of man's physical environment as the significant factors in the appearance of the first civilized communities of the Nilotic region.

Under Weismann's influence environment as an aspect of the problem of development assumed a nebulous outline. For a generation biologists were hypnotized by the discredit of the Lamarckian teaching. Eventually the progress of experimental embryology and cell anatomy relegated Weismann's theory of germinal selection to the same limbo

as the Lamarckian hypothesis. In Weismann's hands the Selection doctrine had assumed a particularly rigid form. Evolution was necessarily a continuous process. All differences between parents and offspring were genetic. Heredity and variation were coextensive processes. From this it followed that a continuous evolutionary process had accompanied the development of social institutions. It was a natural step to confuse the two. The conviction that eugenic legislation is a matter of overwhelming urgency arose as a direct outcome of that step. That the same confusion still dominates eugenic propaganda is illustrated by a statement made by Mr. Lidbetter, a prominent eugenist, in his paper at the World Population Congress of 1927, " It is a platitude ", Mr. Lidbetter stated, " in these days to speak of natural selection as the essential agent in human progress ". It may be a platitude. It is not a truism. It is simply a misuse of terms. Social development is the communication of social tradition and social accomplishment from one generation to another, with the addition of new ingredients in each. Organic evolution is brought about by the transmission through the gametes of new hereditable properties. The mechanism of one is education. The mechanism of the other is sexual reproduction. It is possible that they react upon one another, but the extent to which they do so cannot be ascertained by *a priori* reasoning. The experimental study of genetic variation has made it abundantly clear that evolution is not a continuous process. At present we do not know the precise conditions relevant to the production of mutant types; consequently it is unjustifiable to make any general assumptions about genetic variation in human societies without recourse to direct experimental inquiry.

That is the task which now lies before the social biologist. Its successful accomplishment will not be facilitated by under-estimating the difficulties inherent in the problem. The study of human inheritance is beset by innumerable obstacles. Man is a slow-breeding animal of low fertility. His chromosomes are numerous. The geneticist cannot control his matings. In spite of these drawbacks some insight into the nature of hereditary transmission within the human species can be gained by formulating the results of random mating on certain hypothetical assumptions. Familial studies of colour blindness, brachydactyly and the blood groups provide clear illustrations of Mendelian phenomena. So long as family pedigrees are employed to demonstrate the inheritance of physical characteristics, it is not difficult to recognize the nature of the environmental influences with which the hereditary materials react, and to make allowance for them. The geneticist is on familiar ground. The constituents of man's physical environment have been classified by the physicist, the chemist and the bacteriologist. Their effects upon the physical characteristics of an organism form the subject matter of physiology. It is possible to speak of the action of sunlight and humidity, oxygen pressure and diet, infectious and contagious germs, iodine and calcium salts with some measure of confidence. All these things are features of man's physical environment or of the physical environment of any other animal. The methods for investigating their influence are well tried. The concept of a uniform physical environment is tangible. It can be explained to a pragmatist or a presbyterian, a behaviourist or a bimetallist.

It is another thing to speak about a uniform social environment. The factors which determine man's social behaviour

are obscure and elusive. Even to-day any dogmatism on the relative importance of heredity and environment assumes an almost frivolous aspect when the attitude of the experimental biologist is brought to bear on the evidence. Analogies from the animal kingdom have been pressed into the service of those who emphasize the rôle of either the one or the other. Kropotkin's *Mutual Aid* was the *reductio ad absurdum* of that attitude to social problems. Kropotkin was neither more nor less scientific than the exponents of nature red in tooth and claw. Both were irrelevant. The same irrelevance has been evident whenever biologists have attempted to rationalize their political sentiments. The anti-feminist appeals to the fighting and protective male. The feminist can retort by invoking the worm Bonellia of which the male lives as a parasite in the generative passages of the female. The eugenist pictures the human poultry farm nicely mapped out in pens, each surrounded by its own partition of wire-netting with a few holes here and there. Maybe the Rhode Island Reds have scratched their way into the proper preserve of the Partridge Cochins. Sooner or later the cosmic poultryman, aided by wise statesmen, will put them back where they belong. His opponents can reply that class differences exist in insect communities. The difference between a white ant queen and a termite worker is more striking than the difference between royalty and factory girls ; and it is a difference determined by diet. Encouraging illustrations in support of any social doctrine can be brought forward by those who prefer analogy to analysis.

It might be hoped that the study of human history would assist, but the record of history is ambiguous. A striking instance of this ambiguity is to be found in Professor Carr

Saunders' book on the Population Problem. In the course of a temperate and on the whole well-balanced discussion of the racial factor in history, Carr Saunders remarks that the

> "Nordic peoples are mostly Protestant and the Mediterranean peoples mostly Catholic and Greek. The fact", he continues, "that during the Reformation a choice was set before most European nations as to what religion should be adopted—the issue hanging in the balance for some time in many places—seems to indicate that the conditions were more or less equalized and the adoption of the Protestant religion by the Nordic type was influenced by certain innate characters attaching to that type."

Even if we make a very generous allowance for the genetic homogeneity of the Nordic and Mediterranean populations in mediaeval times, an entirely different interpretation of the same facts is equally plausible. At the time when Christianity received official recognition the countries to which Carr Saunders refers as predominantly Nordic lay on the fringe of Roman Imperial domination or completely outside it. The process of christianizing the Nordic geographical region was still in its infancy when the Holy Roman Empire embarked on its ephemeral and inglorious career. It was hardly complete, when controversey within the Western Church began to assume sinister proportions. With the exception of the Saxons the conversion of the Germanic peoples, including the Frisians, took place in the early part of the eighth century. The official conversion of Saxony occurred about A.D. 800. Christianity was accepted by the ruling powers of Denmark towards the end of the tenth century and by those of Norway and Sweden at the beginning of the eleventh century. The conversion of East Prussia, Latvia and Pomerania occurred during the

twelfth century, and the conversion of Lithuania did not occur until the middle of the fourteenth century. In those countries which Christianity penetrated last of all the conflict between the ruling houses and the temporal claims of the Papacy was generally most acute. Where reformers could seek protection in the clemency of monarchs at loggerheads with the Pope, they spread their doctrines successfully. Where there only existed a religious movement, it was speedily extinguished. The Reformed doctrines spread in those countries where Christianity had been more recently introduced, and where the political sovereignty of the Pope and the economic power of the Church as a landowner were least firmly entrenched and least agreeable to the secular authorities. Catholicism had taken root in the ancient civilization of the Mediterranean region, when the Nordic peoples were outside the pale. If it is true that the Nordic peoples gravitated to Protestantism, it is equally true that they happened to inhabit the geographical region most remote from Rome. There is no reason to suppose that their choice of locality was determined by any characters peculiar to their type or relevant to the progress of theological discovery.

In seeking to make allowance for the significant factors of man's social environment there is no body of accredited information to which the geneticist can turn. There are as many schools of psychology as there are schools of philosophy. The introspective psychologist approaches social behaviour from a purely teleological standpoint, interpreting the means in relation to the end it fulfils. The behaviourist adopts a mechanistic attitude, seeking to interpret the end as predestined by the means. One speaks of a directing intelligence and instinctive action. The other

speaks of intelligent behaviour and unconditioned response. Between the two schools there is a great gulf fixed. It is that which separates the philosophy of Plato from the teaching of Democritus. It is not merely a difference of perspective or of minor issues. Such differences exist in an exact science. The psychologists disagree about the very nature of inquiry into the basis of social behaviour; and there is no immediate prospect that they will come to terms. Meanwhile the eugenist finds himself impaled on the horns of a dilemma. The methods of animal genetics are mechanistic; but the behaviourist is suspicious of the genetical standpoint; while the introspective psychologist fails to define the characteristics of social behaviour in a form suitable for genetic analysis.

When Binet and Terman published their psychological tests, it seemed that there was a brighter prospect for the objective study of mental inheritance. Of late the psychologists themselves have begun to adopt a less confident attitude. Recently the Stanford school of workers have conceded a conservative allowance of 20 per cent. for the influence of home environment on the intelligence quotient. We have no grounds for believing that the ingenious system of home ratings adopted by Miss Burks (1927) in this investigation include all the significant factors. Consequently this figure represents a minimum. The Chicago school have investigated the intelligence quotients of foster children, and adopt an even more sceptical attitude to the value of the I.Q. as a measure of genetic endowment. Tallman has investigated the intelligence quotients of sixty pairs of identical twins. It was found that the mean difference between pairs of brothers and sisters of different ages on the one hand and pairs of non-identical twins on

the other was larger than the difference between pairs of non-identical twins and pairs of identical twins. Accepting the most conservative allowance, it may be stated with some confidence that the contribution of environment to the intelligence quotient is at least as large as the recorded differences between racial and occupational groups subject to different environmental influences.

For two generations eugenists have been writing about mental inheritance. As far as I am aware Professor MacDougall alone has pointed out that the attempt to formulate a concept of mental inheritance raises a very formidable issue which challenges the foundations of current biological philosophy. He himself faces the difficulty by returning to the Lamarckian fold. Lamarck's position was at least consistent. He conceived heredity in mental terms. His theory was teleological throughout. Galton was not consistent, and his disciples have been less so. Since Weismann's time the study of heredity has become more and more explicitly materialistic. To the modern geneticist heredity is one aspect of the physical process involved in the production of a new unit of living matter. His hypotheses are conceived in physical units. The gene has space-time dimensions. Mental inheritance is a meaningless collocation of words, unless it is possible to bring the concept of mentality within the mechanistic framework. That is what the behaviourist school in psychology has undertaken to do. The future of social biology depends on the success which attends their efforts.

Fifty years have passed since Francis Galton published *Hereditary Genius and An Enquiry into Human Faculty*. Since then there have been notable changes in the attitude which scientists have adopted both towards heredity and

human faculty. The work of Mendel, Bateson and Morgan has enormously enriched our knowledge of hereditary transmission in animals. The work of Loeb, Sherrington and Pavlov has opened up new horizons in the study of animal behaviour. The biological analysis of social behaviour presupposes that both methods can be brought to bear upon it. It may be premature to adopt a confident attitude to the prospects, but it is legitimate to state that there is no likelihood of solving the problem which Galton propounded so long as eugenists continue to regard it as the exclusive prerogative of the evoluitonist. The enthusiasms engendered first by the reception of Darwin's hypothesis and subsequently by the spectacular advances which have resulted from Mendel's discovery, encouraged the eugenist to adopt an extremist attitude. New and no less noteworthy developments in the physiology of the nervous system have encouraged the behaviourist to go as far as possible in the opposite direction.

It is not difficult to understand how this has happened. In Galton's time the analysis of animal conduct had not progressed beyond the recognition of those simple units of behaviour which Pavlov calls " unconditioned " reflexes. The scratch reflex evoked on stimulating the lumbosacral region in the spinal dog is an example of this type. Given the same external situation, it can be elicited in any member of the canine species. There are therefore two principal factors which determine the scratch reflex. One is the immediate stimulus. The other is the *inherited* structure of the nervous system. Simple reflexes of this kind play very little part in man's social behaviour ; but modern physiology recognizes a more complex type, which Pavlov calls the " conditioned " reflex. The study of these promises

to meet some of the requirements of a biological analysis of man's social behaviour. The conditioned reflex is not characteristic of all the members of a species subjected to the same immediate situation. It depends upon the time relations of other stimuli which have previously acted upon the organism. Within certain limits it is possible both to predict the outcome, when the time relations of previous stimuli are defined, and to account for a totally different pattern of behaviour in two individuals who inherit the same neuromuscular organization. It was natural that Galton's generation should harp on the hereditary basis of social conduct. They were beginning to understand a type of behaviour in which the genetic factor is the significant variable, and to apply their knowledge to the interpretation of "instinct" in animals. It is not surprising that the behaviourists should adopt the opposite point of view. They are beginning to understand a type of behaviour in which the genetic factor is less important, and to apply the new methods to the study of Man himself.

Even if the behaviourist reaction goes too far in neglecting the genetic aspect of social behaviour, it will have performed one considerable service to social biology. Biology and sociology coincide in the attempt to distinguish what characteristics of human society are related to those characteristics which man shares with all other animals, and what characteristics of human society are related to characteristics which man shares with no other animals. The geneticist is only concerned with the former, since the material basis of inheritance in man and other animals is substantially the same. It is the physiologist who is brought into contact with the characteristics which distinguish man from other animals. Man inherits an immensely developed fore-

brain ; and this circumstance frees him from many of the restrictions which heredity imposes upon the brute creation. The forebrain is the structural basis of conditioned behaviour ; and what distinguishes man pre-eminently from all other animals is the extent to which his behaviour is conditioned by previous experience. A truly biological analysis of human society must build on the recognition that man is the most teachable of animals. This is a profound truth which the eugenist has neglected. The behaviourist has reopened the door which the eugenist closed. The selectionists succeeded in presenting evolution in a form acceptable to their contemporaries. Man was dragged down from his celestial eminence. His place among the brutes became an accepted commonplace of the naturalistic outlook. Sentence had been passed upon him. Henceforth he must live within the prison of his own genetic limitations. Before the portals of his primeval dungeon Heredity stood with a flaming sword. In his new surroundings Man could still demand a retrial, because selectionism was the product of his own forebrain. That trial is still in process. Science has not yet promulgated its final verdict. Galton conducts the prosecution. Watson cross-examines for the defence. Man is released on bail, pending the result of his appeal.

In English law there is a wholesome provision which forbids the public discussion of evidence until the case is closed. In science there is no penalty for contempt of court. It is a pity that there is not. The discussion of the genetical foundations of racial and occupational classes in human society calls for discipline, for restraint and for detachment. Nothing could make the exercise of these virtues more difficult than to force the issue into the political arena in

the present state of knowledge. This is precisely what the eugenist has done. The result is that social biology is encumbered with a vocabulary of terms which have no place in an ethically neutral science; and a growing literature of inquiries repeats all the shortcomings which animal genetics has outgrown. Of these shortcomings anecdotalism is the least. All biologists recognize the disastrous consequences of constructing evolutionary hypotheses on the testimony of the stock breeder and the pigeon fancier. Only an undue haste to establish conclusions which can be made the basis of legislation has arrested the development of social biology in its anecdotage.

Quotations from well-known contributions by eugenic writers will exempt me from the charge of overstating the danger to which I allude, when I speak of the anecdotal method. Few would deny the desirability of shedding further light on the contribution of heredity to feeblemindedness. It is the concern of the social biologist to do so. Goddard's familial studies on this problem have been extensively quoted by eugenic writers. In his investigation several hundred individuals in the Vineland training-school for mental defectives were classified by the Binet test as morons. Goddard conducted inquiries into the family histories of these individuals, and records them in his book. He concludes that a certain type of feeblemindedness is determined by a single Mendelian factor. This conclusion is logically untenable apart from the evidence, because his criterion of feeblemindedness was a segment arbitrarily cut off from a normal distribution curve; but the method which he employs rather than the conclusions he infers is the issue to which I would direct attention. Mendel initiated a new epoch in genetics by clearly defining the nature of the

character which he studied. That practice is the keystone of the science which has developed from his pioneer labours. The Binet test may be legitimately employed as a means of providing an objective definition of feeblemindedness; but since the Binet test is a recent innovation, it is obvious that Goddard could not employ it to identify feeblemindedness in the parents and grandparents of his cases at the time of writing. The method he adopted is stated in the following passage (*Feeblemindedness*, p. 20):

> "The ease with which it is sometimes possible to get satisfactory evidence on the fifth generation is illustrated in the Kallikak family. The field worker accosts an old farmer—'Do you remember an old man Martin Kallikak (Jr.) who lived on the mountain edge yonder?' 'Do I? Well I guess. Nobody'd forget him. Simple, not quite right here (tapping his head), but inoffensive and kind. All the family was that. Old Moll, simple as she was, would do anything for a neighbour. She finally died, burned to death in a chimney corner. She had come in drunk and sat down there. Whether she fell over in a fit or her clothes caught fire nobody knows. She was burned to a crisp when they found her. That was the worst of them, they would drink. Poverty was their best friend in this respect, or they would have been drunk all the time. Old Martin could never stop as long as he had a drop. Many's the time he'd rolled off of Billy Parson's porch. Billy always had a barrel of cider handy. He'd just chuckle to see Martin drink and drink until finally he'd lose his balance and over he'd go'".

At the conclusion of this recital Goddard asks, "Is there any doubt that Martin was feebleminded?"

It may at least be said for Goddard's work that it contains some presumptive indications that genetic factors play a significant part in determining certain kinds of feeblemindedness. It is doubtful whether any plausible conclusions can be drawn from the dreary history of the Jukes. In his monograph on the Jukes in 1915, Estabrook only

ventures to proffer one definite statement concerning hereditary transmission in the Jukes family. It is that " there is an hereditary factor in licentiousness ". I have searched through his memoir for a single indication of the way in which he defines licentiousness and its allelomorphic opposite chastity. Out of a large number of monotonously similar family case histories I shall quote the only one which contains any suggestion of the meaning he attaches to the latter. This (Case G) is as follows:

> " A cousin mating of chaste individuals was followed in the first generation by no licentiousness. In the second generation from the cousin mating no licentiousness appears, although the father of one of the children of this generation had cohabited previous to marriage. Their one daughter was chaste, but she has one daughter brought up in a good home free from bad influences, who was very erotic but is at present chaste. The third child of this cousin mating of chaste people, Addie, married a man who had acquired syphilus and had one son an inefficient syphilitic who died of tuberculosis. Addie died of syphilis at 20. The fourth child Alta V 78 who was always chaste, married but had no children. Horace the only other child of Alfred who reached maturity was reputed chaste but was intemperate: he married a chaste woman and had nine children, all of whom are chaste."

Before we take the risk of wrecking the machinery of social biology by exceeding the speed limit of rational inquiry, it is desirable to ascertain the reasons for such haste. Dr. Estabrook has recorded his own reasons in quantitative terms. " Dugdale estimated a loss to society of $1,250,000 by the Jukes family from 1800 to 1875. The loss to society caused by mental deficiency, crime, prostitution, syphilis and pauperism of these 2,800 people is now estimated at $2,093,685. If the drink bill is added, this total becomes $2,516,685." The reason for this addition

will be more apparent to a prohibitionist than to a brewer. Mr. Chesterton might retort by asking whether there are no idle young clubmen in New York whose annual upkeep is equivalent to the loss entailed by the Jukes during the last century and a half. Deplorable as the history of the Jukes may be, its consequences to civilization may be less disastrous than half an hour's conversation between a manufacturer of armaments and a newspaper proprietor. In such matters private values influence our opinions more than those issues which can be discussed in the public forum of science. Estabrook's arithmetic does not convince me that we should exchange the experimental and sceptical temper of scientific inquiry for the facile slogans of the parliamentary candidate.

The eugenic movement was founded to encourage " the study of agencies under social control that may improve or impair the racial qualities of future generations either physically or mentally ". That aim might be taken as a statement of the scope of social biology, when due allowance is made for the full requirements of a scientific inquiry into the nature of " mental inheritance ". There are a few prominent eugenists who have adhered to this praiseworthy and modest programme. Professor Carr Saunders who has been prominently associated with the eugenic movement in England has consistently expressed himself with discrimination and restraint on the complex issues which the genetic structure of human society involves. If I am disinclined to follow him in the alarmist attitude which he adopts towards the differential fertility which has accompanied the recent decline of the European birth-rate, I entirely agree with him in recognizing that the differential fertility of occupational groups is a matter for careful and comprehen-

sive investigation. To make any satisfactory predictions about the outcome of the present decline it is necessary to ascertain what factors have contributed to the reduction of the birth-rate, what genetic differences distinguish different occupational groups, and how such differences are transmitted. The impressive array of evidence which Beveridge, Stevenson and Carr Saunders have presented strongly suggests that the spread of contraceptive practice has been the main factor in the decline of the birth-rate. The German and Swedish data of Grotjahn and Edin point to the conclusion that contraceptive practice is spreading to all sections of society. If this is so the problem of differential fertility is solving itself. Of genetic differences which distinguish occupational groups we have no definite information. Even if we had, it would be necessary to know how such differences are transmitted before prophesying disaster. Haldane's mathematical analysis of the effect of selection shows that a selective process must be continued for a very long period in order to produce an appreciable effect on the distribution of a character which depends on the co-operation of several recessive genes. An attitude of calvinistic gloom towards the future of human society is not a necessary consequence of the biological study of human society.

In discussing the influence of eugenic propaganda in this essay I have been primarily concerned with the dangers of speculating upon questions whose philosophical importance is less apparent than their practical interest. I trust that I have made it abundantly clear that I am in no sense hostile to eugenics as defined above. Were I to indulge in the luxury of stating a purely personal opinion about the genetics of human society, it would be somewhat as follows.

It is probable that extremes of intellectual accomplishment or defect are significantly determined by genetic variation. It is highly unlikely that extreme types of defective are reproducing disproportionately. It is also doubtful whether genius has ever been biologically fertile. Between the two extremes there is probably a neutral zone in which somatic variability plays a larger part than genetic differences in determining social behaviour. At present it is impossible to assess with precision the mean genotypic endowment of different social groups, whether occupational or racial. Even if it were, the precise significance of the mean would be problematical. I think it highly unlikely that such mean differences as may exist provide any basis for establishing new social barriers or reinforcing old ones, still less for curtailing opportunities of education and the exercise of political responsibility. On the other hand it is not unlikely that there does exist a section of genetic types on the borderline of extreme defect not segregated from the rest of the community and more fertile than others of the same social grade. With Mr. Chesterton I am inclined to doubt whether they represent a larger proportion in one social class than in any other. Unlike Mr. Chesterton I see no reason why society should not deal with this issue as a genetic problem, when it is clearly proved to be a genetic problem. Indeed I think it arguable that it would be wiser not to take any risk of encouraging the feeble-minded to breed. At present I see no way of stopping them.

There can be no disagreement concerning the desirability of exploring every avenue in human genetics. This cannot be done without enlarging the scope of the official census with the support of a sympathetic government. Hitherto

Eugenic propaganda has been dominated by an explicit social bias which, in England, can only serve to render the Eugenic standpoint unpalatable to a section of the community which for good or ill seems to be assuming the rôle of a governing class. The greatest obstacle to the spread of a sane eugenic point of view is the eugenists themselves. By recklessly antagonizing the leaders of thought among the working classes the protagonists of eugenics have done their best to make eugenics a matter of party politics, with results which can only delay the acceptance of a national minimum of parenthood. These last remarks I repeat are a statement of purely personal opinion. Biologists share the human frailty which prompts all of us to entertain beliefs fortified by insufficient evidence; but there is no reason why the biologist should fail to make it clear, when he is speaking as a professional biologist and when he is speaking as a private citizen. From a purely scientific standpoint the problem of human inheritance can only be regarded as a virgin field in which the prospects of an early and abundant harvest are by no means bright. I believe that the eugenists have performed a useful task in emphasizing the need for a biological analysis of human society. The furtherance of that task will not be promoted by propaganda which overstates the achievements of the present, while underestimating the difficulties which lie ahead. Evolutionary inquiry was brought to an end in ancient Greece, when philosophy became the handmaiden of politics. Further progress was checked when philosophy became the bondservant of theology. Eugenics like Greek philosophy derived its first impulse from natural science. It soon entered into alliance with the politician. It is fast finding its most stalwart supporters among the clergy. It can only

realize the aims of its founder by bringing the science of genetics into closer relationship with other methods of studying human biology and annulling the marriage of biological inquiry with political propaganda. As a private citizen the biologist is entitled to his own opinions concerning the merits of sterilizing the unfit, just as he is entitled to his own opinions on the Single tax or the advantages of capital punishment. Such opinions usually belong to his private world. In his public capacity, as a biologist, he is primarily concerned with sterilizing the instruments of research before undertaking surgical operations upon the body politic.

PART III

HOLISM AND THE PUBLICIST STANDPOINT IN PHILOSOPHY

SUMMARY

When the conclusions of physicists are supplemented by the enquiries of the biologist we are led to a schematization of experience which permits us to discuss the nature of matter and life on a neutral ground. This neutral ground is the *public world* of science. It represents what is significant for the purpose of discourse. Idealistic philosophers have assumed the nature of reality as the goal of philosophy ; but the concept of reality is essentially equivocal. For the purpose of discourse we have to assume that the neutral ground is the real thing. In private we are at liberty to reject this view. Temperament decides which of these alternatives we adopt. There is therefore no hope of arriving at universal agreement in discussing the nature of reality. To the introvert reality resides in the domain of mystic experience. To the extrovert the public world is the nearest approach to a complete representation of reality which our limited range of receptor organs permits us to construct. The belief that philosophy can settle the nature of reality, and that it is possible to arrive at universal conclusions independently of the methods of science and mathematics arose in the period of decadence of Greek philosophy. It developed in modern Europe under the influence of ecclesiasticism. Freed from the bondage of clerical control, philosophy must undertake the more modest task of discussing what characteristics of belief determine their communicability or *publicity*, and indicating how the problems of existence can be resolved into their public and private components. From this standpoint educational theory must be based on a recognition of the respective spheres of *publicity* and *privacy*. Organized religious belief and ritual is based on a confusion between the two. The same confusion exists in the pragmatist philosophy.

IX

BIOLOGY AND HUMANISM

"But the mortallest enemy unto knowledge, and that which hath done the greatest execution upon truth, hath been a peremptory adhesion unto authority; and more especially, the establishing of our belief upon the dictates of antiquity. For (as every capacity may observe) most men, of ages present, so superstitiously do look upon ages past, that the authorities of the one exceed the reasons of the other. Whose persons indeed far removed from our times, their works, which seldom with us pass uncontrolled, either by contemporaries, or immediate successors, are now become out of the distance of envies; and, the farther removed from present times, are conceived to approach the nearer unto truth itself. Now hereby methinks we manifestly delude ourselves, and widely walk out of the track of truth."—Sir Thomas Browne, *Pseudodoxia Epidemica*

Evolution does not enable us to make any spectacular predictions which can be verified here and now. Its importance lies in satisfying our curiosity about human origins in a manner consistent with the present state of scientific knowledge. It provides a philosophical framework for biological enquiry on the one hand and for our attitude to the problem of human destiny on the other. From a purely technical standpoint Darwin's specific contribution to the evolutionary doctrine was the hypothesis of natural selection. The hypothesis of natural selection, in the form in which Darwin stated it, has been modified out of all recognition to accommodate later enquiries into the nature of heredity and variation. Such enquiries did not receive their impetus from the *Origin of Species*. They followed the course prescribed by Mendel's experiments upon the kitchen pea. How then has it come to pass that Darwin

has earned a position of pre-eminence only comparable with that of Newton in modern times? Is not the answer that Darwin is the only great natural philosopher who has emerged, since the time of Aristotle, from the ranks of the biologists? When the fullest allowance is made for the inadequacy of contemporary knowledge to meet all the demands of the evolutionary problem, what still distinguishes Darwin's contribution from that of his numerous predecessors in the same field is the consistency and thoroughness with which he set out to explore the implications of evolution in every department of biological information available at that time. By so doing he laid the foundations of a new humanism akin to science, and created a philosophical issue whose magnitude has only become apparent since the rise of the behaviourist school in psychology. Evolution took the discussion of human affairs out of the hands of the humanistic philosophers, and brought it within the legitimate domain of scientific method.

From the dawn of philosophical controversy two opposing tendencies have competed for mastery. One is based upon confidence in the testimony of our receptor organs. The other mistrusts the evidence of the senses. One relies on patient observation. The other appeals to axioms which require no proof. One has its impulse in curiosity about Nature. The other is preoccupied with human obligations. Between the two extremes there have been many makeshifts. As one has fallen into discredit, another has taken its place. The only permanent feature of philosophical discussion is the impossibility of effecting a permanent reconciliation between those who have called themselves, at different periods of history, materialists and idealists, nominalists and realists, empiricists and transcendentalists,

mechanists and vitalists, to emphasize some new aspect of a fundamental incompatibility. In the distinction between publicity and privacy it has been suggested that this antinomy does not necessarily reside in the system of nature. It arises because our curiosity exceeds our information. Up to a certain point we succeed in pooling our experiences by the method of science. In so doing we construct the public world. The method of science has achieved its most conspicuous success in dealing with inanimate objects and with the brute creation. To the few who are genuinely interested in these things the extent of our knowledge has always seemed of more importance than the magnitude of our ignorance. Hence there have always been philosophers of a type which, for want of a better term, may be called *materialistic*. The majority of people are not interested in natural objects except in so far as the knowledge which science confers contributes to their personal comfort. Man is pre-eminently interested in himself. So long as science cannot satisfy that curiosity his private and personal values assume a greater importance than the academic hypotheses of science. Philosophers who teach us to distrust the guidance of our sense data flatter our egotism, and soothe our vanity. Science cannot. On the other hand science can supply us with aviation, broadcasting and twilight sleep. Transcendental philosophy can only offer us the good life. The politician distrusts the mechanist, because Science provides no concept of divine right to fortify social privilege. He cannot go all the way with Parmenides and identify the way of the senses with the way of error, because scientists are too useful. The only philosophy for the plain man who wants a plain answer to a plain question is some sort of compromise between the standpoint of transcenden-

tal metaphysics and mechanistic science. Since the sixteenth century humanistic philosophy has been dogged by the problem of accommodating man's interest in the world around him with his interest in his own person. Worldly interests compel civilized man to recognize the claims of science up to a certain point. Egotism prompts him to demand some supernatural sanction for the vagaries of his own social conduct. For three centuries traditional philosophy has been haunted by the possibility that science might in the end succeed in satisfying man's curiosity about his own nature. Darwin made that possibility the explicit concern of scientific enquiry. From mediæval times all attempts to effect a reconciliation between the empirical standpoint and transcendental values have been concerned with defining separate spheres of influence in which science and metaphysics can operate without mutual interference. On that basis philosophy is still taught in our universities to-day. Darwin's *Descent of Man* challenged the complacent dualism which had permitted humanistic philosophy and utilitarian science to pursue an independent course from the Renaissance to the middle of the nineteenth century.

At every stage in the advance of scientific knowledge a new system has arisen to conserve some fragments from the wreckage of supernatural beliefs. There have been three outstanding attempts to effect a compromise between observation and inspiration. The systems of Aristotle, of Descartes and of Kant each exhibit features characteristic of the state of contemporary scientific knowledge. Each has a peculiarly interesting significance in the history of biological science. The system of Aristotle was the last will and testament of Greek biology. The system of

Descartes was finally overthrown by the evolutionary theory. The system of Kant is predestined to a similar doom, when professional philosophers are prepared to face the disquieting consequences of modern research on the " labyrinthine organ " and Sherrington's work on the " muscular sense ". If it is too early to predict the fate of holism, it is instructive to reflect on that of its predecessors.

§ 2

Aristotle undertook the task of finding a place for science in a civilization in which scientific enquiry was approaching its decline. His problem was simplified by the circumstance that the conflict between teleology and mechanism had arisen as a conflict of interest rather than interpretation. Burnet insists that the idea of purpose only emerged at a comparatively late date in the history of Hellenic science, and the more recent researches of Cyril Bailey emphasize the same aspect of the incompatibility of natural and moral philosophy in ancient Greece. Greek philosophy had inherited from the Hesiodic cosmogony the belief that Chaos was the beginning of all things, gods and men alike. It was unfettered by the Chaldean Fall and the doctrine of creative providence. Aristotle's system was therefore an attempt to harmonize two tendencies which had come into being independently of one another. One begins with Thales. It bore fruit in the brilliant speculations of Leucippus and Democritus. It survived after Aristotle's death in the mechanical discoveries of the Alexandrian school. In the poem of Lucretius it made a final gesture of august rhetoric. To modern civilization it bequeathed the indestructibility of matter, and the atomic concept which was

revived by Gassendi in the seventeenth century. Its rival was anti-scientific and mystical. It diverted enquiry from the observation of nature to the duty of man. It was preoccupied with the good life, with the soul and with the hereafter. It began with the Eleatics and the Pythagoreans. It gave birth to the school of Plato; and its influence survived long after the extinction of Hellenic civilization. Through Philo Platonism transmitted the Logos to the Nazarenes, and at a later date the neoplatonists equipped the Church with the apparatus of the Trinitarian controversy. It completed its contribution to Western culture in the Council of Trent, when the mystery of human life, death and duty were placed for all time on a firm foundation of deductive reasoning.

The success of Platonism was assured by the powerlessness of Greek materialism to satisfy the requirements of Greek politics. The plain man wanted to know whether the wise man will obey the laws if he knows that he will not be found out? With transparent honesty Epicurus could only reply that a simple answer is difficult to find. The benign and tolerant humanism which Epicurus grafted on the soil prepared by the atomists was ill suited to flourish in the stern climate of the military state. Like holism, Aristotle's system was a shrewd blending of science and statesmanship. It enabled its author to combine a personal predilection for natural history with a political partiality for slavery.

Aristotle borrowed from Plato the doctrine which identifies the *good*, or real nature with the best or most normal condition of a thing. He rejected the respect for mathematics which Plato borrowed from the Pythagoreans, and incorporated Plato's teleology in his theory of the physical

world. One disastrous effect of this may perhaps be seen in the neglect and preservation of more ancient work. Empedocles is known to have made observations on respiration and the movement of the blood. It would seem that he had recourse to experiment of a crude kind. One fragment of his writings contains a hint that he came very near to anticipating Torricelli and Harvey. The details have been lost. Empedocles also put forward a theory of vision. He held that the eye contains fire. Sight is produced when the fire within the eye goes forth to meet the object. A hypothesis so well adapted to the view which interprets life by "correlating the initiation of the activity with its end" was, as might be expected, immortalized by Plato to the calamity of physical and physiological enquiry. Science had a long road to traverse before men learned the truth of Nietzsche's statement that "the most valuable knowledge is always discovered last, but the most valuable knowledge consists of methods".

The fundamental incompatibility of the naturalistic and Platonic attitude to the Nature of Life is illustrated by the following passage from the writings of a contemporary philosopher, Professor Wildon Carr:

> "It is wholly inadequate to classify natural objects into the inert and the living, into objects which are not responsive and objects which are responsive to external impressions, and then to seek to specify the property or character which differentiates the one class from the other. Life is a perfectly definite and distinctive phenomenon. It is not a thing, neither is it the character of a thing. It is a purposive activity exercised within clearly ascertainable limits and having a definite range. I use the term purposive without any implication of awareness. Living activity is purposive in the meaning that it can only be understood by correlating the initiation of the activity with the end. The nature of the activity is plainly recognizable, however difficult it may be to conceive the agent,

> agents or agency which the activity implies. . . . Life is individual: it exists only in living beings, and *each living being is indivisible, a whole not constituted of parts.*"

The concluding remarks of this passage are somewhat reminiscent of the Athanasian creed, the germs of which were actually derived from the neoplatonists. It displays the sterilizing influence of the Platonic teaching in a peculiarly explicit form. If the words quoted in italics were actually true, experiments carried out daily by medical students in physiological and pharmacological laboratories would be impossible. The physiologist takes the living machine to pieces and studies the properties of its several parts. The experimental embryologist can put it together again. Ross Harrison grafts the head end of a tadpole of one species on to the tail end of a tadpole of another species. The progress of modern biology has been made possible by the implicit rejection of the attitude which Professor Wildon Carr advocates; and this is not merely true of the growth of experimental physiology as a quantitative science. In our generation Bergson has rehabilitated the evolutionary doctrine in teleological language. Its history very clearly shows that the rise of the evolutionary hypothesis in its modern form is traceable to the liberation of biological enquiry from Aristotelean teleology. Two centuries of research, fertilized by the experimental temper which Vesalius reintroduced into the study of medicine, paved the way for a new school of naturalists in the eighteenth century. Ray and Linnæus took up the comparative study of animal life where Aristotle had left it. At a later date Cuvier, Milne Edwardes and Owen founded a school of comparative anatomy which—unlike post-Darwinian morphology—made the issue of experiment the final court of appeal. It was

thus that Darwin and Wallace brought to the discussion of Man's place in Nature a mass of new data, accumulated in the process of displacing the teleological bias under whose influence Greek biology steadily declined. It is futile to blame Aristotle for the influence which his particular form of compromise exerted on the history of biology. In Aristotle's time it was impossible to say: "Here is a property of living matter, the teleological attitude leads us to make such and such predictions; quantitative analysis leads us to contrary conclusions: let us submit the merits of the two methods to the issue of experiment". Aristotelian biology was not an experimental science.

When Aristotle infused into Greek biology the teleology of Platonism, natural science had already progressed as far as it was destined to advance without coming into conflict with teleological presumptions; but the inevitability of the conflict was not yet apparent, and does not emerge as a clearly defined issue in Greek philosophy. Greek materialism, like modern science, was the offspring of secular curiosity. For this reason it is easy to trace the germs of modern hypotheses in the speculations of the ancients. It is more difficult to determine how far such analogies are merely verbal. The extent to which the Greeks had recourse to experiment has given rise to lively controversy. We are more familiar with their beliefs about nature than with the evidence on which they relied. To reconstruct the scientific knowledge of Anaximander and Empedocles from the fragments of their writings which remain is a task no less formidable than that of a future historian who has nothing but the torn pages of a Harmsworth encyclopædia to provide evidence of contemporary science. The fact remain that the scientific knowledge of the ancients lacks two

features which are highly characteristic of modern enquiry. Greek science was static. Having no means of measuring short intervals of time, the Greek could make little use of dynamical notions. The mathematical technique for dealing conveniently with dynamical calculations was not evolved until Greek geometry was supplemented by the universal arithmetic of the Arabs. Galileo's dynamics was the death-blow to Aristotelian teleology. To the Greeks time relations were philosophical enigmas. They had not as yet become objects of scientific enquiry. Modern mechanistic biology lays emphasis on *process*. Greek biology was still dominated by *structure*. This preoccupation gave rise to the distinction between form and substance which assumes a prominent place in the Aristotle of the schoolmen, and persisted in modern science, until the dynamical concept of electromagnetic mass was put forward. A second characteristic which distinguishes contemporary science from the natural philosophy of the Greeks has been emphasized less by historians of science. This may be because astronomy and mechanics were developed by the school of Alexandria in close association with mathematics. That brilliant but short-lived phase in the science of antiquity was post-Aristotelian. Astronomy was the only branch of natural science which had attained the precision of measurement in Aristotle's time.

The distinction between Aristotelian vitalism and the modern biological standpoint is most apparent in the attitude which Aristotle adopted to quantitative investigation. " Mathematical accuracy of language ", he declares, " is not to be required in all things, but in those things that do not involve any connexion with matter. Wherefore such is not the natural (alternatively *physical*) mode of discovering

truth, for perhaps the whole of Nature involves matter. Therefore first must we investigate what Nature is. For in this way also will it be evident about what only natural science is conversant, and whether it is the province of one science or many to speculate into causes and first principles." This passage occurs in the first book of the Metaphysics. There are other passages in which the same point of view is expressed. They show conclusively that Aristotle was not content with ignoring the connexion between mathematics and science. He definitely asserted that the quantitative study of natural phenomena is not the correct one. In this he went further than Plato. While Plato despised the study of physics and biology, he recognized the vital dependence of astronomy on mathematics and valued its pursuit. The standpoint which Aristotle adopted is comprehensible. His interest in nature was mainly that of the naturalist. He did not study living creatures in the belief that by so doing he would be able to predict their behaviour. That possibility only emerged into prominence when Lavoisier and Laplace brought the thermometer and the balance to the study of respiration. By then the spectacular progress of physics had necessitated a new adjustment of the claims of moral and natural philosophy. The philosophy of Descartes liberated biology from developing within the limitations prescribed by the Aristotelian system.

Two events contributed significantly to the situation which Descartes faced at the beginning of the seventeenth century. The period which intervened between Aristotle and Descartes witnessed the rise of Christianity and the origin of a new cultural synthesis within the world of Islam. At Alexandria natural science and mathematics still flour-

ished, when Christianity became the official faith of the Roman Empire. Five centuries after the death of Aristotle Diophantus made what seems to have been the first definite contribution to the development of algebraical analysis. Alexandrian science came to an end when "on a fatal day in the holy season of Lent", Hypatia the expositor of Diophantus "was torn from her chariot, stripped naked, dragged to the church, and inhumanly butchered by the hands of Peter the reader and a troop of savage and merciless fanatics". Two centuries later what was spared by the religion of Cyril was consigned to the flames by the victorious armies of Islam.

While Christianity and Mohammedanism were competing to appease the deeper needs of mankind, the genius of a dark-skinned people was preparing the stage for a rebirth of European culture. Before the destruction of a second library of Alexandria the Greek learning had filtered into the middle East through the hospitality which the Persian court extended in turn to the banished Platonists, the Jews and the heretic Nestorians. In India Brahmagupta and his successors had applied themselves to the same problems which Diophantus had assailed. Half a century before the foundation of the University of Baghdad Hindu astronomical tables and the rhetorical algebra of the far East had been introduced into Persia. By the beginning of the ninth century the works of Ptolemy, Euclid, Hippocrates and Aristotle had been translated into Arabic. At the end of the tenth century the Moorish culture was firmly established in Europe. In the universities of Cordova, Seville and Toledo, the study of medicine and mathematics flourished side by side. The medical schools of Italy and France were outposts of the Moorish culture in the twelfth and thir-

teenth centuries. The Moors extended the Alexandrian pharmacopœias; and advanced the study of anatomy. Jewish missionaries of the Moorish culture brought the tradition of dissection into Italy. From Arabic manuscripts Italy first made contact with the resources of Greek science. Arabic learning transmitted the texts from which ecclesiasticism imbibed its taste for metaphysics, and at the same time contributed two influences subversive to the Aristotelian system. It gave birth to chemistry as an experimental science, and it developed the beginnings of syncopated algebra. The invention of algebra made possible the efflorescence of physics when Greek geometry had long since completed its task as the midwife of mathematical astronomy.

From the fact-loving temper of Aristotle the naturalist Arabic curiosity had absorbed all that could be used as a basis for scientific enquiry. In the Sorbonne the Platonic ingredients of his system brought fresh grist to the mill of theological controversy. By seeking for "causes and first principles", Aristotle in his own words had made philosophy the "divine science". As the divine science it was pressed into the service of orthodox theology and the Protestant revolt against ecclesiastical authority. During the fourteenth and the fifteenth centuries the resources of the Eastern Empire were ransacked for the rhetoricians and sophists neglected by the empirical disposition of Arabic scholarship. The political speculations of the ancients became the rationale of Protestant democracy. As the influence of Platonism on science succumbed to the success of the new experimental and quantitative methods which were gaining ground in physics, classical humanism completed the divorce of moral and natural philosophy by

elevating the authority of Plato in politics: but before classical humanism had gained ascendancy in the mediæval universities, an ascendancy which became a monopoly in the grammar schools, Arab science had implanted in the oldest seats of European learning a seed destined in the fullness of time to challenge not merely the authority of church councils but the authority of print, to place the authority of experience above the authority of the written word, as Protestantism had placed the authority of the written word above the authority of church councils.

Plato would have had little use for physicists in his Utopia. To outlaw science was the last thing which Protestant rulers were prepared to do. The great navigations had made science a necessity to the mercantile interests. So long as they refrained from making themselves a nuisance, like Servetus whose inability to envisage the Trinity in its correct numerical proportions earned for him a harsher fate than that of Galileo, scientists must be left alone. The world had outgrown the Aristotelian compromise. The time had come to replace it by some new device.

The Cartesian philosophy met the new situation by defining separate spheres of autonomy for the scientist and the metaphysician. It did not attempt to mix metaphysics and science in a uniform system of nature. With his propositions and demonstrations " which establish the existence of God and the distinction between the mind and body of man disposed in geometrical order " Descartes stumbled upon the felicitous notion that God ordained the investigation of nature according to strictly mechanistic principles. " Now that I know him," he discloses in Meditation V, " I possess the means of acquiring a perfect knowledge respecting innumerable matters, as well relative to God Himself

and other intellectual objects as to corporeal nature, in so far as it is the object of pure mathematics."

So piety prescribes that the scientist, contrary to the admonition of Aristotle, must apply mathematics to the investigation of nature. By the same process of reasoning or inspiration Descartes arrives at the conclusion that teleological hypotheses in natural science are an impious abrogation of the prerogatives of Deity. " Likewise finally," he asserts in the *Principles of Philosophy*, " we will not seek reasons of natural things from the end which God or nature proposed to himself in their creation (i.e. final causes), for we ought not to presume so far as to think that we are sharers in the counsels of Deity, but, considering him as the efficient cause of all things, let us endeavour to discover by the natural light which he has planted in us, applied to those of his attributes of which he has been willing we should have some knowledge, what must be concluded regarding those effects we perceive by our senses ; bearing in mind however what has been already said, that we must only confide in this natural light so long as nothing contrary to its dictates is revealed by God himself."

As Descartes was prepared to concede to mechanistic science the entire brute creation, the Cartesian framework provided plenty of latitude for biological investigation. Descartes stimulated experimental physiology by his own ingenious speculations ; and his influence on physiology survived after it had been superseded by later philosophical systems. In biology the Cartesian tradition has fallen into discredit through the rise of evolutionary ideas. The evolutionists persisted in following " the natural light " ; and their theological contemporaries maintained that it led them to conclusions contrary to what had been " revealed by

God Himself ". Since then biologists have been faced with the alternative of pressing forward to a more radically mechanistic conception of life, or abandoning it altogether. I have called the extension of the mechanistic conception the *publicist standpoint* in contradistinction to *holism*, the most recent compromise.

In *Science and the Modern World* Dr Whitehead calls the period immediately before the Renaissance the Age of Reason. That which followed is the Age of Faith. The Age of Argument and the Age of Confidence might perhaps describe the difference in more significant terms. Confidence in the method of science has grown gradually with the expansion of civilization. What is called modern science has arisen, because innumerable, at first independent, lines of enquiry have coalesced. It has not come into being because the men who have pursued these separate but converging paths have had any general theory about nature as a whole, purposeful or otherwise. Curiosity has always induced some men to speculate without investigating, others to investigate without speculating beyond their terms of reference, and a good many to do both. Aristotle was neither the first nor the last scientist to write a natural history and a volume of Gifford lectures. Newton divided his time between gravitation and the prophecies of Daniel. Faraday elaborated a theory of the ether and advocated the tenets of Sandemanism. Aristotle's natural history, Newton's gravitation and Faraday's ether were not the philosophical complements of their respective ethical, political or devotional beliefs. Aristotle dissected animals, Newton gazed at the stars, Faraday experimented with his electrical machine, because they were interested in living creatures and the heavenly bodies and electrical phenomena.

The pursuit of science has its personal impulse in curiosity and its social impulse in the power which science confers. The Greeks accepted or rejected as illusory the evidence of their senses. In Aristotle's system science does not beg for a philosophical sanction. It was reserved for the piety of Descartes to introduce the singular idea that the scientist requires a licence to practice signed and stamped by the metaphysician. The idea that science requires a metaphysical justification has been revived by Dr Whitehead who adopts a novel, and I think incorrect, view of Hume's contribution to philosophy. When the scientist enters the field of contemporary philosophical controversy, he is confronted with a jargon whose existence has very little historical or logical connexion with the assumptions on which he works. It is not surprising that he remains, in the words of Dr Whitehead, " blandly indifferent " to the arguments with which Hume refuted those notions of causality and inference which traditional philosophers have chosen to regard as the justification of science. The important fact about Hume's argument is that he refuted the pretensions of moral philosophy by the same arguments which demolished the irrelevant dogmas which natural philosophy inherited from the age of scholasticism.

Starting from a purely introspective basis Descartes attempts to find a justification for the growing confidence of mankind in the testimony of the senses. The subjective empiricism of Locke is still groping after the same solution. Hume pursues this method to its bitter conclusion ; and shows that it led to complete scepticism. He proves to his own satisfaction that the method of introspection cannot be made the basis of socially communicable knowledge. The theory of a public world builds upon that foundation,

by examining the characteristics of questions to which a socially communicable answer is possible. The strength of Hume's position is more apparent in our own generation than it was at the time, when he wrote. Our expectation of living has increased as we have learned to worry less about the good life and more about the good drain. That the questions with which science deals are legitimate objects of enquiry and that the method which science adopts is the most satisfactory way of answering them is tacitly accepted by everybody who avails himself of the amenities of a railway time table, the bioscope, the telegraph office, the diphtheria vaccine, the ocean liner and the air mail. If we do not attempt to answer the questions which perplexed Socrates, it is because we have at our disposal a vastly greater body of material to guide us in determining what characteristics of a question make it suitable for being asked. We cannot say whether the wise man will obey the laws, if he thinks that he will not be found out ; but we are less interested in knowing the answer, because we know that it is highly probable that his finger prints will be traced. Detective fiction has familiarized us with other devices which have replaced the deathbed confession of the repentant atheist.

Hume applies the Cartesian method, demonstrates its sterility, and arrives at this attitude which he states with no uncertain sound in a passage which occurs at the conclusion of the essay on " The Sceptical or Academical Philosophy ". " When we run over libraries persuaded of their principles, what havoc must we make ? If we take in our hand any volume of divinity or school metaphysics for instance, let us ask, Does it contain any abstract reasoning concerning quantity or number ? No. Does it contain

any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames. For it can contain nothing but sophistry and illusion."

In Hume's scepticism natural science is no longer the man with the muck rake, and metaphysics is no longer the divine science. Kant's critique now appears to salvage supernaturalism by a compromise in which the Cartesian method is reversed, and a greater confidence in scientific method is evident. He invokes science to save the soul of man. Newton's system had made space and time the basic concepts of science. The physiology of Kant's generation had not extended its enquiries beyond the range of the five senses with which the Greek empiricists were familiar. Kant seized upon this limitation of eighteenth-century biology to substantiate the contention that science draws upon information which is independent of sensual experience. In Kant's idealism space and time, the basic concepts of science, appear as purely mental constructions. This was not a new point of view; but the prominence it assumed was new, and resulted from the character of contemporary science and the conclusions which Hume had advanced.

Volumes of speculation on the relation of time and space to sensual experience have been written both by physicists and metaphysicians on the assumption that the human frame possesses no means of recording its own rhythms and its orientation to the earth's gravitational field. Mach is the only natural philosopher who has hinted at the possibility that the Kantian argument might be re-examined from the modern biological standpoint. Modern physiology is not circumscribed like the physiology of Kant by the five senses. It recognizes ten or eleven distinct types of

receptor elements in the human body. Two of these are immensely significant to the attitude which we adopt to Kant's criticism of Hume's position. In 1828 the experiments of Flourens first demonstrated that animals are receptive to the influence of gravity. The receptive area in our own bodies is located in that part of the internal ear, sometimes called the labyrinthine organ. Just as removal of the eye prevents a fish from responding to its background by colour change, destruction of the labyrinthine organs abolishes its characteristic orientation in space, when swimming. We do not say that a cat falls on all fours, because it has a priori knowledge of space relations. It falls on all fours, because the orientation of its body as a whole is recorded by its labyrinthine organ, and the appropriate muscles are brought into play by reflex action. The possibility that orientation of individual members with reference to one another might be regulated by a self-recording arrangement is a comparatively recent discovery. Sherrington has shown that the tendons and muscles possess special structures which he calls proprioceptors. They respond to the stretching of the muscles. By virtue of those muscular rhythms which Galileo employed as his standard of reference in devising the first clock, the human body is itself a self-recording timepiece. Pavlov does not appeal to a priori knowledge of time to interpret the interval which elapses between the ringing of a bell and the secretion of saliva in a dog.

The belated discovery of the labyrinthine organ and the proprioceptors is easy to understand. We live in a world in which day and night follow one another. We can close our eyes and darken our vision. The eyes are exposed to view. Their connexion with light calls for no elaborate

demonstration. The labyrinthine organ and the proprioceptors are only accessible to dissection or to the microscope. We can never get away from the influence of gravity or the rhythms of our own bodies. What we never miss, we fail to notice. This peculiarity of our corporeal nature gives a peculiar and fallacious plausibility to Kant's contention that we can take away from a body its colour, weight or smell, all of which can be "referred to mere sensuous experience", while "the space which it occupied still remains and this is utterly impossible to annihilate in thought". To annihilate space is not an impossible feat of imagination for the modern biologist. In Rupert Brooke's poem the heaven of fishes harbours the worm that never dies. If the metaphor had been pushed a little further, it would have transpired that the philosophy of fishes might well contain many axioms that are omitted in the Kantian critique. Fishes in general have no eyelids. They can be kept in a uniformly illuminated aquarium without the experience of darkness. The sharks and their allies possess a small duct by which the internal ear communicates with the exterior. It would not be an impossible operation to remove and replace the contents of their labyrinthine organs, and render them temporarily indifferent to the earth's gravitational field. A philosophical fish confined from birth to a uniformly illuminated aquarium and subjected from time to time to this simple operation, might conceivably invert the Kantian argument. He would be shocked by the gross materialism of an undulatory theory of light; but he would be willing to be convinced about any quaint views upon space which the piscine physicists propounded.

§ 3

Kant's doctrine that space and time are concepts necessarily independent of one another and of sensory experience has only come into conflict with the fruits of scientific enquiry in our own generation. In spite of this the influence of Hume has steadily increased and the influence of Kant has declined during the intervening period. Psychology, brought into existence by Kant's appeal for a science which would define the characteristics of a priori knowledge, has betrayed its parent. In every department of human enquiry investigators are asking, does it contain any abstract reasoning concerning quantity or number, does it contain any experimental reasoning concerning matter of fact or experience? Mr Bertrand Russell, an impenitent advocate of a priori knowledge in his capacity as a mathematician and an uncompromising empiricist in his capacity as an educationist, tells us that "logic must no more admit a unicorn than can zoology, for logic is concerned with the real world just as truly as zoology". Dr MacDougall, the most vigorous contemporary critic of the behaviourist standpoint, is devoting his energies to the breeding of genetically pure stocks of Wistar Institute white rats. A professor of political science in London University so far departs from the traditions of his discipline as to collect statistics on the number of hours which members of Parliament actually devote to their democratic activities. It was said by an old Scots balladmonger, "I care not who makes the nation's laws, while I sing her songs." When the modern descendant of Democritus is assured that materialism is on the decline, he may well reply in a similar vein. We all behave as if we were behaviourists nowadays.

Between Hume and Darwin, Adam Smith, Malthus,

Quételet and Marx sowed the seeds of the behaviourist standpoint in the soil of the humanities. Darwin's *Descent of Man* precipitated a new phase in the revolution which was replacing classical humanism by a humanism which draws its inspiration from the success of natural science and looks to the scientist to supply it with the first principles of method. It was no longer possible to challenge the claim of science to the study of the brute creation. The recognition of man as a by-product of the same secular agencies undermined the last defence of the metaphysician. Hegelian philosophy was a forlorn and belated attempt to lock the stable doors after the horses had long since escaped. The piety of Descartes had conceded that animals are automata. The brutal candour of Darwin emphasized in a new light the fact that man is an animal. It was fitting that William James should arise in the fullness of time to proclaim the new gospel that philosophers are also men. If the full force of this devastating sequence was not apparent to the pragmatic apologists of the Pelagian heresy, it has acquired a new, and, I believe, epochal significance through the breakdown of the Cartesian distinction between reflex and voluntary activity in modern physiology.

To-day the influence of Hume's empiricism and Darwin's doctrine of descent can be seen in every branch of social enquiry. In history the materialistic bias is evident in the writings of those who would be least willing to commit themselves to economic determinism as a general hypothesis of social development. Economics has completely severed its moorings to moral philosophy, and proudly boasts that it is an ethically neutral science. The genetic aspect of human behaviour is recognized in new endowments to encourage the pursuit of social biology. If social psychology

has hitherto remained immune to the influence of modern research on animal behaviour, it borrowed its equipment of instincts from the discredited speculations of the selectionists. The humanities have passed out of the hands of the grammarians in the higher seats of learning. Before another generation has passed the accomplished fact will be officially sanctioned in the earlier stages of education.

In its long apprenticeship to theological dogma classical humanism has created a type of philosophy which is inimical to the temper of scientific enquiry. This tradition has been perpetuated in our educational system by associating the study of history and human affairs in general with an exclusively linguistic rather than with a scientific training in early life. The belief that philosophy lies outside the province of science and that logic is more fundamental than science has been so thoroughly inculcated by the classical tradition that most men of science accept it with servile complacency. It is not surprising that many of them view with alarm the magnitude of the new territories which scientific method has incorporated within its domain. In every period the birth of a new tradition has made the forces of reaction more stubborn. This fact must be accepted. It is unwise to expect results of far-reaching importance in social science in the immediate future. If astronomy is the most exact of the observational sciences, it is also the oldest. It began in Babylon and Egypt about six thousand years ago. Economists and eugenists who hope to outgrow their phlogiston theories in one generation take an unduly hopeful view of the rapidity of scientific progress. New forms of compromise will come to the rescue of custom thought and power thought, before social science

is able to replace the wrong way of asking a question by the right way.

It is highly probable that the next century will witness a consolidation of supernaturalistic tendencies in western Europe. It is legitimate to entertain the possibility that the rival attitude, reinvigorated by fresh triumphs of scientific method in the treatment of human affairs, will revive with greater vitality, if not in our own civilization, in that which takes its place. I am in complete agreement with Dr Haldane, General Smuts, Professor Eddington and Dr Whitehead, when they assure us that the materialistic tendency in philosophy, which was gaining ground under Darwin's influence, is less popular to-day. I venture to interpret the reaction in a way with which they would not agree, and to believe that it has retreated *pour mieux sauter*. The mid-nineteenth century in Great Britain was a period of prosperity and expansion. In Huxley's generation unbelief was the luxury of a privileged class which was not afraid of the man in the street. The period in which we live is one of ferment and disintegration. In its impetuosity to settle the problems of human conduct, it will not be content to await the slow advance of science. Mechanistic philosophy cannot offer to the privileged a supernatural sanction for the things they value most. It cannot proffer to the unprivileged the shadowy compensation of a world into which the thought of science is unable to penetrate. A mechanistic philosophy might conceivably be popular in a society in which gross inequalities of possession did not exist. To-day it can only flourish among those who have leisure to study, when their privileges are not compromised by social unrest. He who has the temerity to defend the mechanistic position need not expect any laurels from his

own generation. He cannot seek sanctuary in the fearless candour of a contemporary Huxley or a contemporary Tyndall. He must extract what comfort he can glean by reflecting that the system of Aristotle triumphed over that of Epicurus, and the thought of the nineteenth century was nearer to Epicurus than to Aristotle. Scanning the new blossoms which have lately been added to the nosegay of philosophic compromise, he will say with Swinburne,

"But for me their new device is barren, the times are bare,
Things long past over suffice, and men forgotten that were."

X

PUBLICITY, REALITY, AND RELIGION

"Conscientiousness in small things, the self-control of the religious man, was a preparatory school for the scientific character, as was also, in a very pre-eminent sense, the attitude of mind which makes a man take problems seriously, irrespective of what personal advantage he may derive from them."—Nietzsche, *The Will to Power.*

§ 1

WHATEVER may be said against pragmatism, it served a useful purpose in calling attention to the connexion between people's temperaments and their philosophies. William James classified philosophers in two genera, the tender-minded and the tough-minded. I believe that there exists a more fundamental distinction between two types which correspond to the introvert and the extrovert of the psychiatrist. Our attitude to the scope of philosophy is determined by whether we take an individualistic or a social view of what constitutes truth. In the one case our canons of logic will be inferred from an examination of the properties of propositions which carry conviction to ourselves individually, in the other by an examination of the properties of propositions which are best equipped to obtain general assent. For this reason I am not convinced that Bertrand Russell is right in thinking that modern logic brings the most devastating criticism to bear on traditional philosophy. Modern logic has not been devised with the aim of achieving the same results as those which constitute the goal of traditional philosophers. It is not surprising that traditional philosophers, finding its conclusions unpalat-

able, remain unconverted. Modern logic is the offspring of mathematics, and mathematics has developed in intimate association with science. For the last seven hundred years traditional logic and introspective philosophy have been no less intimately associated with theology. Hence has arisen the arrogant assumption that on account of its subject matter introspective philosophy is more fundamental than science. There is no reason why we should regard it in this light. If philosophy is knowledge, it is presumably something transmissible by discourse. Otherwise it is difficult to formulate any distinction between knowledge and mere opinion or superstition. We cannot therefore discuss knowledge without taking into account the people who share it. The most important characteristic of scientific beliefs is their communicability. Those who take the social as opposed to the individualistic attitude to truth will embark on their adventures by examining the characteristics of scientific judgment. To any one who is not an incorrigible individualist the results of scientific enquiry must establish the anatomy of philosophy. Whether I am by temperament an individualist or otherwise I am forced to submit to the discipline of discourse in practice. Let us suppose that two individuals N and M are engaged in public discourse concerning the Nature of Life. N states the proposition "I (N) am a conscious being". M states "I (M) am a conscious being". The only neutral ground for the discussion of the two statements is the more general statement "N and M are conscious beings". In the nomenclature suggested elsewhere in these essays this neutral ground is the resultant of the public components of the two original statements which N and M make. Personal statements may be looked upon as complex variables. Philosophy is

the technique of operating with the real (public) part of such statements and separating them from the imaginary (i.e. private) part. This distinction is not abolished by the fact that the temperaments of some philosophers lead them to apply the term *real* to the private and *imaginary* to the public component. The fact remains that in public discourse we have to operate with the public component. The public statement " N and M are conscious beings " implies our ability to define characteristics of behaviour which are denoted by the adjective *conscious*. This task which now lies within the scope of biological investigation refines the publicity in the concept of consciousness. For the performance of public discussion the term " *I* " is a member of the class denoted by " *Man* ". Anything implied in public discussion by the statement " I am conscious, etc.," is included in the statement " Man is a conscious animal ". The significance of anything which cannot be subjected to this limitation is a purely *private* matter. From this it follows that a discussion of the Nature of Life is complete, when we have taken into account the characteristics of conscious behaviour. On the contrary view the propositions of philosophy are not necessarily communicable.

The theory of a *public world* suggested in a previous essay on the Nature of Life corresponds very closely with the attitude adopted by Poincaré in the following passage which occurs in his *Foundations of Science* :

> " Sensations are therefore intransmissible or rather all that is pure quality in them is intransmissible and foreever impenetrable. But it is not the same with relations between these sensations. From this point of view all that is objective is devoid of all quality and is pure relation. . . . Nothing therefore will have objective value except what is transmissible by discourse . . ."

So far as it goes there is nothing essentially new in this statement. It represents an attitude common among thoughtful people with a scientific training. Professor Eddington is in agreement with it when he writes:

> "For reasons which are generally admitted, though I should not like to have to prove that they are conclusive, I grant your consciousness equal status with my own; and I use this second-hand part of my consciousness to 'put myself in your place'. Accordingly my subject of study becomes differentiated into the contents of many consciousnesses, each constituting a *view-point*. There then arises the problem of combining the view-points, and it is through this that the external world of physics arises. Much that is in any one consciousness is individual, much is apparently alterable by volition; but there is a stable element which is common to other consciousness. That common element we desire to study to describe as fully and accurately as possible, and to discover the laws by which it combines now with one view point, now with another. This common element cannot be placed in one man's consciousness rather than in another's; it must be in neutral ground—an external world . . . The external world of physics is thus a symposium of the worlds presented to different view-points . . ." (p. 283, *The Nature of the Physical World*).

How then does it come about that "a universal Mind or Logos would be, I think, a fairly plausible inference from the present state of scientific theory"? (p. 338). Or again, how are we to draw the conclusion "from those arguments from modern science that religion first became possible for a reasonable scientific man about the year 1927"? I think that we may possibly find an answer to these questions by bearing in mind that Professor Eddington's external world is the external world of physics rather than the public world of physics plus biology. Throughout his exposition he assumes that biologists are still committed to the dualistic standpoint of the Cartesian tradition.

Thus he states: " a mental decision to turn right or turn left starts one of two alternative sets of impulses along the nerves to the feet. At some brain centre the cause of behaviour of certain atoms or elements of the physical world is directly determined for them by the mental decision . . ." (p. 312). It is not clear why there is in what Professor Eddington calls the " mystical experiences " of different people sufficient " neutral ground " to provide the basis of a symposium with as universal a sanction as that of the external world of physics. That after all is implicit in our notion of religion. It is not difficult to see why he finds no objection to placing his external world under the direction of a universal mind, since he assumes that the teleology, which has been abandoned in dealing with non-living matter, is quite indispensable in dealing with living matter.

It must be admitted that Professor Eddington could claim the support of at least one eminent biologist who holds that the method of the biologist is different from the method of the physicist, and the method of the physicist can never be applied to the analysis of " conscious behaviour ". However stoutly Dr Haldane may advocate the first proposition his many distinguished contributions to biological science are little calculated to illustrate its truth. He has made important additions to our knowledge of the physical chemistry of the blood by adopting the method of the physicist. When, if ever, he has departed from that method he has done so without the assent of his fellow physiologists. The practice of even the most exemplary persons not infrequently falls short of the loftiness of their professions, and it is perhaps unfair to criticize Dr Haldane on this account. The weakness of Dr Haldane's philoso-

phical position resides in the fact that he gives no consideration to the new issues raised by the physiology of the conditioned reflex. Pavlov's work has shown us that even when he is dealing with " conscious behaviour ", the biologist can still approach the subject matter of his enquiries with the same attitude which the physicist adopts.

On the whole people are more interested in conscious behaviour than in anything else. Before science could attempt to tell us something about conscious behaviour, Poincaré's outlook could never have a far-reaching appeal. The history of human thought again and again proves that people will always fall back on the language of magic, when the language of science provides them with no vocabulary in which to discuss the things that interest them most. Magical views of the world have declined not because science disproves them, but because science provides better ways of discussing the same issues. If we can usefully treat the characteristics of conscious behavour without invoking a holistic or animistic concept of consciousness, the scope of introspective philosophy must in time dwindle to a vanishing point. Philosophy will then confine itself to examining the logical structure of scientific theories. It may seem more natural, more in keeping with common sense, to think of a wholeness defined by the term consciousness than to face the tremendous intellectual effort of envisaging the behaviour of an organism from an atomistic standpoint. If the latter contains within it the capacity of growth and of yielding verifiable conclusions which cannot be derived from the traditional point of view, consciousness must go as gravitation and action at a distance must go, however much Kant and common sense may urge the *a priori* necessity of a Euclidean space and a measure of time that is

independent of it. As scientific investigation invades the domain of conscious behaviour the way will be open for developing a new outlook in philosophy, one that is neither intrinsically monistic nor intrinsically pluralistic, since it makes no such claims to finality as the academic philosophies of the past have usually done.

§ 2

Professor Eddington, like Professor Whitehead, entertains the hope that science may eventually lead us to conclusions about the universe involving something more than practical utility on the one hand and mere intellectual satisfaction on the other. The reasons which they give are not sufficiently definite to criticize, as they would perhaps themselves admit. It is more easy to understand the point of view of Mr Sullivan, an impenitent individualist, who is frankly resentful towards science, because science cannot serve the needs of theology or provide sanctions for his æsthetic predilections.

> " The greater importance that men attach to art and religion ", he maintains, " is not due simply to their ignorance of science. Art and Religion satisfy deeper needs ; the problems they deal with are intrinsically more important. . . . Our æsthetic and religious experiences need not lose the significance they appear to have merely because they are not taken into account in the scientific scheme. . . ." [1]

The real significance of these remarks would be much clearer, if Mr Sullivan had substituted the word *personally* for *intrinsically*. Mr Sullivan is by temperament an individualist philosopher. For him what is *true* has a specially personal value. Scientific beliefs are only *convenient*. Thus

[1] *The Bases of Modern Science*, pp. 234–5.

in discussing the system of Copernicus he makes the comment, "it was convenient; the question of whether it was true or untrue was not explicitly discussed".[1] Consistently with the individualistic standpoint in philosophy Mr Sullivan does not divulge the inward revelation which empowers him to distinguish with such nicety between propositions that are "true" and propositions that are merely "convenient".

It sounds very impressive to state that science leaves out of account man's religious and artistic experiences, that religion and art satisfy deeper cravings and so forth; but religion and art are two words which are rarely used by any two people in the same sense or by any one person in the same sense on two successive occasions. Which religion does Mr Sullivan mean? Is he a Buddhist, a Seventh Day Adventist, a Shintoist, or a Bahai? Is his religion an ethic or a cosmogony, or both? Until he has defined his position more explicitly, it is difficult to be quite sure what he is talking about. The terms art and religion are used in very different senses. That being so, to say that everybody has religious or æsthetic experiences does not necessarily imply the existence of any common plane of thoughtful intercourse other than the conceptual world of science. An expert social hostess recognizes this when she wisely refrains from asking Mr A who is interested in Art to meet Mr B who is interested in Art.

In seeking to transcendentalize his private world, Mr Sullivan has not been altogether felicitous in coupling together Art and Religion. Although many people become very irritable in the course of a discussion on the merits of vorticism or free verse, most educated persons admit on reflection that such discussions owe their interest to the

[1] *The Bases of Modern Science*, pp. 234–5.

light they shed on differences of temperament in the disputants. It is true that professors of literature in Universities and rather youthful reviewers sometimes take a more pontifical view of their own powers of divination, but even our schools have got beyond the stage, when it was thought proper that children should be whipped into believing that Wordsworth's *Idiot Boy* is a *great* poem. Whether Mr Sullivan chooses to say that Van der Waal's equation is true or merely convenient, it may be suggested that he would embark on a discussion of its truth or convenience with some hope of final agreement. I believe that Mr Sullivan would embark upon an argument about a question of æsthetics with much less hope of changing his opponent's attitude than if he were discussing Van der Waal's equation. Mr Sullivan has no need to appeal for a transcendental sanction for æsthetic experience. Asethetics are questions about which sophisticated people agree to differ.

With religion it is different. Whereas a person can have very genuine artistic interests without claiming a universal, transcendental or *public* sanction for his own preferences, religion ceases to be religion and becomes æsthetics or ethics, when it does not put forward such claims. A consideration of the following illustration will make this clear. A derives satisfaction from reciting Mr Yeats' poem, " The faeries dance in a place apart . . ." etc. B derives satisfaction from singing the evangelical hymn that contains the lines " Bright crowns there are, bright crowns laid up on high . . ." Inasmuch as the first form of satisfaction involves neither belief nor unbelief, it is properly described as artistic or *æsthetic*. Inasmuch as the second implies in addition the conviction that a certain ponderable object exists at a certain height from the earth's

surface, as in the cruder forms of evangelism it does, the satisfaction derived from singing it is *religious*.

The word religious is used in so many different senses that it is dangerous to employ it at all without examining it more closely. Cynical paradox-mongers not infrequently complain that materialists are " religious ". When so used, the term merely implies a sense of the importance of belief. In an unscientific age persons of this type would probably gravitate towards some form of religious organization. Looking at religious organizations as a whole one sees two sharply contrasted components in the majority: views about human conduct or the ethical aspect of religion and views about the nature of the universe or cosmogony. To-day, most educated people regard the latter as the proper sphere of science. We are told by modernists that the earlier chapters of the Book of Genesis are to be cherished for the sublime ethical teaching they impart. What sublime ethical teaching is implied in the prohibition to eat of the fruit of the tree of Knowledge or in the story that woman was manufactured from a man's rib as an afterthought of creation need not detain us. It is only partly true to say that liberal theologians have surrendered the sphere of cosmogony to science. They have surrendered the details, but they have not surrendered the prerogative of imposing upon whatever cosmogony the scientists supply a teleology of their own. This teleology is, it is true, so attenuated as to embarrass the advance of science far less than those more crude revivals of animism sometimes inappropriately classified under the generic heading New Thought. Its justification becomes less and less calculated to carry conviction, as the domain of scientific method is officially sanctioned by the Churches. The advance of scientific know-

ledge reinforces the suspicion that an attitude to experience which leads to misleading or sterile conclusions about the details of our cosmogony can hardly be expected to prove essential to the finished picture.

Just as the cosmological aspect of religion has become resolved into a *public* component which has passed over into the province of science and a personal component for which the mystic seeks to formulate some ulterior transcendental sanction, the ethical side of religion consists of values which must be taken or left and prohibitions or admonitions which can be rationally discussed as instruments for promoting the acceptance of these values, if the latter are taken for granted. Here again the claims of liberal theologians become more modest with the advance of more complex methods of social organization. Sunday observance, fasting, prohibitions against dancing, smoking and theatricals become less and less fashionable. Even in the domain of sex, where animistic views of human conduct are more obtrusive, we find that Anglican deans are making the entertaining discovery that birth control is fully compatible with the teachings of Christianity. Most Christians will agree that war is contrary to the " spirit of Christ's teachings ". When a war happens to be in progress, they give no objective evidence that this conviction differentiates their conduct in any way from that of people who are not in the least interested in the spirit of Christ's teaching. An exception might here be made in favour of the Quakers. The Society of Friends, the only religious body which engaged the respect of Voltaire, have shown so little disposition to proselytize that they are hardly to be looked on as a religious body. They might with equal propriety be called an organization of persons interested in the art

of living. Significantly enough they do not call themselves a Church.

In practice the hard and fast ethical claims of religious leaders tend with the advancement of civilization to become less pretentious. The details of regulating human conduct have been conceded to the politician and the educationist, just as cosmogony has been surrendered to the scientist. Religious organizations cling tenaciously to some obscure transcendental sanction for the fundamental assumptions on which the politician or educationist is expected to work, assumptions which, as earlier remarked, seem in actual practice curiously irrelevant to the behaviour of their devotees. The increasing vagueness which surrounds the nature of " revelation " in the teachings of Liberal Churchmen renders the task of making this sanction communicable one of overwhelming difficulty. In relation to human conduct there is obviously a large domain of questions on which rational discourse is possible, in so far as the fundamental assumptions are agreed upon by all parties. In this sense ethics belongs to the public world and ethics and politics are the same thing. There still remain private differences with regard to the premises. The Roundheads realized that transcendental ethics cannot be made the subject of argument. They acted intelligibly on the assumption that the only answer to the Divine Right of Kings was to make a spectacle of the head of Charles Stuart to Gods and men.

When Professor Eddington employs the term mystical experience indifferently for æsthetic and religious sentiments, he is perfectly justified in so far as the ultimate constituents of religion, those residues of religious belief which have survived the secularization of social life and the advancement of scientific knowledge, belong to the

private worlds. From a purely individualistic standpoint the "religious" satisfaction that the Liberal theologian derives from the crude teleology of the Chaldæan mythos is difficult to distinguish from the artistic satisfaction sought by others in the vague teleology of Wordsworth's *Tintern Abbey*. Regarded from a social angle there is a profound difference between the religious and the artistic experience. Because of that difference Professor Eddington's conclusion that there is nothing in the outcome of scientific enquiry to prevent a reasonable man from entertaining religious beliefs is a profoundly misleading one. When Professor Eddington speaks of religious experience he clearly means something which belongs to himself privately. When the vast majority of people speak of religion, they mean a body of beliefs which can be transmitted through the medium of discourse like scientific beliefs. Far from being regarded like artistic preferences as a private affair of the individual, such beliefs are promoted by exceedingly powerful organizations which still exercise an immense effect on social behaviour. This influence takes the form of interference on the part of the Churches in every attempt to encourage birth control, to promote true information about sex among the young or to humanize the divorce laws. The scientific philosopher is entitled to his own private mythos with which no sensible people would wish to interfere, provided that he does not pester his fellows with it. It is still permissible to ask whether he is justified in employing language in such an equivocal manner that his words will be used, when he must know they will be used, to give all the weight of a distinguished reputation to those forces of social organization which in the past have exercised a constant restraint on the freedom of scientific enquiry and do at present exert

a tremendous influence upon the shaping of human conduct.

That the distinction we have drawn above between the religious and the æsthetic is the significant one for the purpose of ordinary usage is easily seen by considering the dislike that some people entertain for pork. In a Gentile this is an æsthetic attitude. In the Jew it is a religious one. The thesis that artistic values, or more generally what Professor Eddington calls the mystical experience, belong to the private world requires an important qualification. It does not imply that Art cannot be made the subject of discussion. A mechanistic philosophical outlook is often stigmatized on account of its supposed dullness, and shunned because it seems to leave no place for after-dinner conversation. This is not so. The mechanistic outlook does not imply the end of æsthetic criticism. It merely insists that such criticism shall conform to the standards of the public world. There is abundance of fascinating problems dealing with the orientation of human interest to particular objects which come within the scope of æsthetic criticism. The theory of a public world which has been developed elsewhere leaves open the possibility that we may one day have genuinely scientific knowledge about these things. If that day comes, we shall be able to argue about art and ethics without losing our tempers. As the behaviouristic standpoint encroaches on the field of art criticism, it is probable that the nice distinction between those matters of taste which we call æsthetic values and those which we call ethical sanctions will seem more arbitrary than the advocates of Art for Art's sake postulate.

The conflict between religion and science tends to be obscured by the circumstance that the official apologists

of the former, if they are well-educated persons, state their case in such a way as to convey the impression that they only claim that their view of human destiny is a permissible one. This does not alter the fact that religious leaders both of the right and left wing *behave* on the assumption that their views have a universal sanction. In very few parts of the English-speaking world is it possible for a child to go to school without being taught the tenets of some religious body. One does not notice that Liberal theologians who state their case on the frankly private basis of mystical experience are much more sympathetic than Fundamentalists to the legitimate claims of the agnostic parent who wishes his child to have a secular education. The term religion cannot be detached in its objective, or in the terminology of these essays its *public*, aspect from *organization*. Professor Eddington has justified his right to a " mystical experience ". He has not proved his claim to have a *religion* as the average parent understands that term. If the scientist uses the term *religion* for something different, knowing that his words will be used by religious leaders to reinforce their claims to a universal sanction for their own " mystical experiences ", the secularist parent may legitimately feel that the scientist is not giving him a square deal as a fellow citizen.

§ 3

We must attribute to its long association with theology the idea that philosophy deals with something mysteriously called *Reality*, lying outside the secular province of science. The term Reality has acquired the value of magical gesture in academic philosophy. Its everyday use as a measure of the intensity of one's conviction throws a good deal of light

on what is meant when it is used to define the goal of philosophy. To the introvert the private world is most *real*. To those who have a more socialized attitude to experience the public world of science is most *real*. The fundamental difference that exists between the introvert and the extrovert type of philosophy is not abolished by introducing the word reality into the language. It is partly because the term "external world" has been used with more emphasis on its "reality" than on its communicability that I have preferred to speak of a *public world*. The classification of different experiences as external or internal is less important than the recognition that some are communicable and others less so. In this nomenclature scientific beliefs are distinguished especially by their *publicity*. The fundamental distinction between the domain of intellectual public enterprise and intellectual private enterprise is just as valid, whether we approach the question from the frankly solipsistic standpoint adopted by Professor Eddington on p. 268 of his Gifford Lectures or the equally explicit objective idealism of p. 272 of the same work.

In my undergraduate days there was a legend of an eminent philosopher and a Fellow of Trinity. Returning to his room in the early hours of the morning after a liberal potation of audit ale, he lay down upon the hearth-rug, covering himself with one of the large shallow footbaths which were still used by all classes of academic society. When his bedmaker arrived a little later, he explained that he was an oyster, and raised objections to any one tampering with his shell. I do not know whether the story is true, or whether the philosopher was a solipsist. I presume that in the course of the same day, he realized that his experience of the footbath had a more extended significance than

his experience of the oyster shell. If he were a solipsist and decided to remain such after the incident in question, he was presumably forced to recognize from Audit eve onwards, that certain ingredients of his consciousness had a more permanent status than others. If human beings exist only in my own individual " consciousness ", they constitute necessary points of reference in classifying other types of experience. The distinction between *the* public world and *my* private world does not uniquely owe its usefulness or significance to any assumption concerning the existence of a reality external to my own consciousness. The important feature about the world construction of science is not its externality but its communicability. Communicability remains a perfectly definite basis for the classification of beliefs, even if I choose to deny that other human beings have an independent existence.

In secret the individualist is entitled to cherish the belief that his own private world is more " real " than the public world of science. One suspects that Mr Sullivan does so. Professor Eddington is evidently worried because he finds himself doing so. Under the influence of love or alcohol we have all been solipsists at some time or other. As Professor Eddington has lucidly stated in a passage quoted earlier in this essay, so soon as we engage in public discourse we are compelled to seek for a neutral ground. We agree to leave our private world behind. To make discourse possible we accept the neutral ground as the real thing. This neutral ground is the public world of science. The idea that philosophy is more fundamental than science has arisen through the absent-mindedness of philosophers. This permits them to overlook the fact that there cannot be philosophy unless there are philosophers. As soon as more

than one individual begins to philosophize the search for a neutral ground becomes a necessity of social intercourse. From a social point of view the neutral ground is the only thing which can be spoken of as real. Although philosophers often try to give the impression that human beings exist in virtue of the fact that there is such a thing as philosophy, it is more sensible to hold that philosophy exists in virtue of the fact that there are human beings. Because philosophers are themselves members of human society, the proper goal of philosophy must be the search for propositions that have the property of *publicity* or socialized reality.

The recognition of this restores to philosophy an intelligible objective. From the point of view of the average person the philosopher is "a blind man in a dark room chasing a black cat that isn't there". The contempt of the plain man is partly justifiable, not because the plain man is entitled to a plain answer to every question he propounds, not because philosophers are blind, but because it is waste of time to chase the cat, if there is really no cat to chase. Traditional philosophy wedded to dogmatic theology has always assumed that the cat is there to chase. Of late years the cat of traditional philosophy, like the cat of *Alice in Wonderland*, has been gradually disappearing. For the purpose of apologetics there is little left of it but its smile. There have been philosophers who have been content to admit their blindness and refrain from putting forward any project to bell the cat. Of such was David Hume. It is now a hundred and fifty years since Hume was buried. This may be why he is recognized as an authentic philosopher. Those whose studies lead them to entertain views somewhat similar to those of Hume are more usually called anthropologists, physicists, physiologists,

economists or generically experts, while they remain alive.

The grudging benediction accorded to Hume's remains by traditional philosophers is not wholly due to a conventional respect for those who have departed. Hume divested natural philosophy of some of the pretensions which it had imbibed from its association with scholasticism. Philosophers with an anti-scientific bias have chosen to regard this as an affront to science. The scientist looking back over a century and a half of unparalleled progress has no need to regard it as such. On the other hand the scope of "moral" philosophy has dwindled since Hume's time. Psychology, until our own generation a branch of moral philosophy, is clamouring to be recognized as a science. Anthropology has undertaken the task of elucidating by painstaking observation those aspects of human behaviour which are *publicly* connected with what Professor Eddington *privately* speaks of as the "mystical experiences". It may be true that scientists in our generation are less outspoken than Huxley and Tyndall in their criticism of traditional philosophy. It may be true that those scientists who enter the field of philosophical discussion often do so with the aim of reinforcing the beleaguered battalions of the apologists of dogma. It is doubtful whether there was ever a time in the history of western Europe when a secular outlook was more widespread, and when the hope of finding a rational basis for a universal religion was less forlorn.

When philosophers speak of a rational basis for scientific belief, they seem to imply that the word rational can only signify that which is evident independently of experience. It is profoundly doubtful whether we can form any judgment about such a question. If it is possible to arrive at a decision of this kind, I suspect that the solution will come from

those who study the behaviour of infants and the history of science. There seems to be little hope of obtaining a solution from introspective philosophers. In the meantime science will continue to progress, whether the belief that relations between experience can be ascertained is a rational one or is itself an outcome of experience, whether the public world is the actual world or a shadow world, whether the conclusions of science appeal to common sense or seem more incredible than fairy tales. In a machine-made civilization however unpalatable to common sense the conclusions of science may prove, the future of science is assured.

In the philosophy of Hume we find the pragmatic justification of science first stated explicitly. " To philosophize ", according to Hume, " is nothing essentially different from reasoning on common life ". Kant's anxiety to give scientific enquiry a " rational " sanction was based on the frank recognition that Hume's scepticism threatened the future of moral philosophy more than the future of science. Yet Kant himself could not escape from the pragmatic criterion of publicity, in asserting the superiority of his own system to that of Plato, who " abandoning the world of sense, because of the narrow limits it sets to the understanding . . . did not reflect that he made no real progress by all his efforts, for he met with no resistance which might serve him for a support, as it were, whereon to rest, and on which he might apply his powers."

In accepting Hume's critique of traditional rationalism and attempting to reinstate moral philosophy on the same footing of social convenience as natural science, the modern pragmatists have proved more than they intended. By insisting on the temperamental basis of philosophical belief, pragmatism has robbed moral philosophy of all claim to

universality; and implicitly relegated it to the status of an art. James believed that the doctrine of immortality is a physiological necessity for some people. For others it is evidently not. Some people anticipate with gratification an eternity of hymn singing. Others shudder at the fate of the Struldbrugs. Both those who do find it necessary to believe in immortality, and those who do not, live to-day as citizens of a society whose amenities are the fruit of scientific knowledge. In a machine-made civilization the amenities provided by science are a necessity to every one. It is necessary to live in order to philosophize. When the philosopher has finished all that he has to say about the Nature of Life, it is the biologist who is called in by his relatives to certify that he is legally dead. The universality of science transcends in a very practical sense those differences of temperament which determine the predilections of moral philosophers. Scientific hypothesis makes social activity possible.

XI

PRIVACY, PUBLICITY, AND EDUCATION

"Nothing does more harm in unnerving men for their duties in the present, than the attention devoted to the points of excellence in the past as compared with the average failure of the present day."—Whitehead, *Science and the Modern World*

§ 1

Two conclusions, it seems to me, can now be drawn from the progress of science. One is that, since we can never know everything we should like to know, every individual has a right to his own private world. The other is that there is no excuse for the sophisticated person refusing to recognize where his private world ends and the domain of social knowledge or the public world begins. From the first it follows that there is no necessary antagonism between the claims of science and art in a modern theory of education. From the second it follows that true education is necessarily secular. It is generally agreed that education includes something more than vocational training. Modern industry offers to the majority of people the prospect of more opportunities for cultivated leisure. It is arguable on the other hand that as time goes on work may become more rather than less monotonous for most people. Training of the individual to use his leisure in ways which will not bring him into conflict with his neighbours provides a possible basis for the public discussion of cultural education in what may be called its æsthetic aspect. The Theory of the Public World does not necessarily imply that such discussion is valueless. It does not lead to the conclusion

that the æsthetic side of education is unimportant. It does necessitate a reconsideration of the attitude which the teacher should be encouraged to adopt. A modern theory of education should begin by defining the respective spheres of *privacy* and *publicity*.

I shall illustrate what I mean by privacy with special reference to the teaching of poetry in the school. I can see no good reason why a child should be expected to like Keats' *Ode to a Nightingale*. I can see excellent reasons why he should know that it was written, and where to find it. One of the characters in a play by Eugene O'Neil is made to say : " I love dynamos. I love to hear them sing. They're singing all the time about everything in the world." Mrs Fife was an American. She had almost certainly never heard a nightingale sing. In England, where the nightingale is an indigenous species, its overrated vocal performances are hardly less familiar to the average child. To the majority of people brought up from childhood in urban surroundings dynamos are closer to Nature. They are things that they can see and hear. Churchyard owls and beds of asphodel belong to books rather than to life. For the average citizen it may be valuable to know that the word Hippocrene does not mean the same thing as hippopotamus. From every point of view enlarging the vocabulary is an important part of education. Enlarging the vocabulary and developing an interest in literature as a means to cultivated leisure in adult life are entirely separate issues.

I once asked a friend who is a biologist whether he was interested in modern poetry. He hesitated and replied that he quite liked *Evangeline*. I am almost certain that he had not opened Longfellow's works since he was a schoolboy, unless he had received from an aunt one of those

editions which are bound in mauve suède and sold for Christmas and birthday gifts. He was no less truthful than most of us. He displayed a motor reaction which can be evoked from many people who have been educated on the assumption that the teacher's business is to cultivate " good taste ". If one of our leading dailies were to make favourite English poets the subject of a prize competition, it is safe to predict that Wordsworth would receive many more votes than William Blake. It would be interesting to ascertain the number of hours devoted after school age to the perusal of the works of Wordsworth and Blake by those who would vote for one or the other. I know of no one who has undertaken this task as a thesis for the Ph.D. degree ; but I would hazard the surmise that the aggregate would favour Blake rather than Wordsworth. The modern teacher has abandoned the exegetical method in favour of the " play way ". But the advocate of the play way is no less certain that if he likes Shakespeare, his pupils ought also to like Shakespeare. If he is really interested in Shakespeare, and has an engaging personality, he may succeed so long as they remain under his influence. It does not follow that the pupil will carry into adult life the means of enjoying his leisure in a way that will not be a nuisance to his neighbours.

In discussing the æsthetic aspect of education, it is difficult to exclude the intrusion of one's private values. I am aware of this difficulty. Anything which I proffer to the discussion is of a very tentative nature. The cultivation of taste involves two separate issues. Conventionally it signifies the existence of some fixed standard of correct enjoyment. The cultivation of good taste in this sense is only justifiable if we have satisfied ourselves that philosophy

can provide a rational sanction for the affirmations of æsthetic experience. If we have reached the conviction that the affirmations of æsthetic experience have no status in the Public World, we have no justification for interfering with another person who reads John Oxenham's *Bees in Amber* in preference to Conrad Aiken's *Punch the Immortal Liar*, or the works of Miss Ella Wheeler Willcocks in preference to the sonnets of Edna St Vincent Millay. I think experience would sustain the statement that people who habitually read Coventry Patmore or Francis Thompson are temperamentally different from people who prefer to read Osbert Sitwell or Carl Sandburg. I suspect too that such temperamental differences exist even in early childhood. They are therefore outside the realm of argument or education. On the other hand, experience convinces me that many would read poetry in adult life, if they had not been repelled in childhood by the sickly romanticism or pedantic archaicism of those writers who are usually exhibited as models of good taste. Possibly a number of people who read Mr Kipling would not do so if they had access to more sophisticated forms of entertainment. Many others do not read poetry and rather despise those who do, because they have never realized that the subject matter of poetry is not necessarily circumscribed by a holistic view of sex or confined to the domestic affairs of the minor Greek deities. There is a legitimate sense in which taste can be cultivated. We may go through life impoverished, because we have never been introduced to sources of satisfaction which might enrich our experience. It is the proper function of the educationist to acquaint us in early life with a great variety of opportunities of socially agreeable behaviour. Having done this, he or she must leave us

to select what is most appropriate to our temperamental peculiarities.

The technique of education in its æsthetic aspect has received less thoughtful attention than its more practical problems. After a succession of ludicrous experiments, educationists now realize that the attendance of art galleries is not increased by forcing children to draw one white cone, a pyramid and two cubes piled up on a drawing-board. It is also recognized that a weekly period devoted to the tonic sol-fa notation does not make a nation musical. I would suggest that the disappointing results of æsthetic education are pre-eminently due to the fact that educationists have never recognized that æsthetic values do not belong to the public world. I would go further and suggest that more positive results might be achieved, if our educational practice were founded on a recognition that æsthetic preferences come within the proper domain of what I have called *privacy*. Education has too long been dominated by the æsthete who regards his own values as having some final and transcendental sanction. Because temperaments differ, æsthetic education can never leave a permanent impress on the majority of people, so long as it is dominated by a school of private opinion, whether that school is Shakespearian or Shavian, Realist or Vorticist. The rising influence of science, if it checks the influence of pontifical æstheticism is calculated to reinforce rather than curtail the æsthetic aspect of modern education.

In *Science and the Modern World*, Dr. Whitehead has maintained the contrary view. He affirms that " in regard to the æsthetic needs of civilized society the reactions of science have so far been unfortunate ". I do not think that there is any historical justification for this assertion.

During the Renaissance there was a very intimate connexion between the progress of biological science and the development of painting and engraving. Human beings and horses had been represented with some measure of biological fidelity in Greek plastic art. But realistic treatment of animals and plants in general was a late development of the Art of the Renaissance. Most of the descriptive biology of Aristotle, of Theophrastus, and of the Arabs was a dead letter when it filtered into modern Europe. The descriptions in the Greek and Arabic herbals were too indefinite to be adequate for purposes of identifying species without the aid of good illustrations. Anyone who will take the trouble to refer to the originals of Conrad Gesner's *Natural History* or Gerrard's *Herbal* will appreciate the statement that Dürer initiated a new epoch in biology by depicting a recognizable rabbit. It was no fortuitous circumstance that Leonardo da Vinci was both a distinguished anatomist and a distinguished painter. The naturalist of the Renaissance had to be an artist. There were no cameras. The tradition of Realism in Art developed side by side with the progress of medicine to meet a need which no longer exists.

To-day we have cameras, and Realism in Art is declining. I venture to suggest that the invention of cinematography, though still in its infancy, is unfolding new artistic horizons. It is still common to find that educated people disregard its latent possibilities as Puritan England despised the stage. It is unwise to assume that Victorian ideas of Art have more finality than future generations will in all probability ascribe to them. Silas Marner, who had never seen a skyscraper or a dynamo, could not be expected to like the *Cabinet of Dr Caligari* or Capek's *R.U.R.* For the same reason we should not expect Whitechapel school-children

to enjoy Landseer and the Lake poets. To the Victorian æsthete a machine and a factory were necessarily ugly. The Immortal Stagyrite had settled the criteria of beauty two thousand years before. It is worthy of note that American industrialism rarely impresses the visitor with that drab monotony which is so characteristic of the English town. This may be partly because American education has been less dominated by the æsthetic predilections of a pre-scientific era in human history. The increasing importance of science in education need not react unfavourably on its artistic function. If it has done so, it is not because science has at any time dominated our conception of a cultural education. The cultural value of science has hardly been recognized as yet.

§ 2

However defined, education includes fitting the individual for some kind of productive activity. On this account the place of science as a part of vocational training is now securely entrenched in our educational system. The hard logic of economic necessity has forced us to make more and more concessions to natural science. It is doubtful if more than a handful of educationists see clearly that those changes in the structure of civilization which have necessitated such concessions have made scientific study of paramount importance in cultural education. The terms cultural and vocational are, by many people, still used coextensively with literary and scientific. It is true that science is not directly concerned with the æsthetic side of education. If in addition our definition of culture includes an intelligent orientation to human society, ignorance of science is incompatible with a cultured outlook in

the present age. The special features of modern civilization depend on the extent to which scientific knowledge has been applied to the conquest of Nature by mankind. The picture of the physical universe which science offers for our contemplation is therefore the nucleus of what is socially most vital in our time.

The task of giving science a place in our conception of cultural education presupposes a good deal more than the addition of obligatory science subjects to the curriculum. Chemistry is usually taught in universities so as to ensure that the student will be able to discharge with competence the work of an analyst in the public service. It is also possible to teach the same subject in such a way that the student gets some glimpse of the adventure of scientific knowledge, some insight into the method of science as a way of dealing with human experience, some apprehension of the challenge which throughout the ages science has issued to comfortable beliefs and established traditions. This attitude to the study of science is still rare among those who have been educated on exclusively scientific lines. The classically educated person can at least be said to have something which was once a culture. He has a more or less consistent attitude to the world around him. In spite of all that is said about the menace of scientific materialism, a consistent mental attitude is very rare in scientific men. They usually have two attitudes, one for the laboratory, one for Sundays and the domestic circle. This is partly because scientific education has been almost entirely vocational in its emphasis.

That it is still necessary to emphasize the place of science in cultural education is a heritage of the humanistic revival. Until comparatively recent times the leading educationists

of western Europe were agreed that the cultural side of education is satisfactorily accommodated by a study of two dead languages. The founders of the Grammar Schools were men who after a life-long devotion to Latin and Greek had found in the classical authors a link with the living past and a real source of æsthetic delight. Their pupils rarely progressed sufficiently far with the rules of accidence and syntax to acquire any genuine knowledge of classical literature or ancient history. The number of modern Englishmen who enjoy Scandinavian drama or Russian fiction through the medium of translated works is certainly greater than the number who deliberately read the classical tragedians in the original. Leaving out of account professional historians, the number of people who have been inspired to learn more about ancient history through dipping into Mr Wells' *Outline* is probably greater than the number of those who as schoolboys acquired a taste for history from construing Xenophon, Thucydides, Cæsar and Quintus Curtius Rufus. The grammarians, in the words of Mr Wells, were fumbling with the keys of the past to open the doors of a ransacked treasure chamber. It is now widely recognized that the results of classical education have been disappointing. In relation to the æsthetic side of education, it cannot be said to have promoted the growth of what Dr Whitehead calls " a living art which moves on and yet leaves its permanent mark ". Socially it started at the wrong end. An intelligent orientation towards society presupposes a knowledge of the history of human society. It also presupposes on the part of the individual a vital appreciation of his own surroundings. Unfortunately, the obsolescence of classical education is not the result of a reasoned conviction of its cultural inadequacy. The influ-

ence of the grammarians declined because they could not meet the practical requirements of our age.

Science came to occupy its present status in the school curriculum as part of a comprehensive change in educational outlook associated with the rise of the manufacturing class to political power. The aristocratic tradition in education, with its humanistic bias towards formal logic, Latin and Greek, sufficed so long as the Church and Law were the principal professions which attracted the sons of the well-to-do. The coming of the machine age opened up new horizons of professional activity necessitating prolonged and highly specialized training. With the development of new international communications came a greater demand for acquaintance with the living languages of a nation's customers abroad. Initially the demand for science in education was justified on purely vocational grounds, a fact which has given scientific education on every step of the educational ladder a fundamentally utilitarian tendency. In our own time the demand for biological instruction as a school subject has been very largely motivated by a utilitarian objective. The public is told how important it is that our future citizens should realize that by studying the domestic habits of the mosquito biologists have made it possible for engineers to construct the Panama Canal. It is further argued that if our future citizens were brought up to entertain a more lively respect for them, in short, to give them a greater measure of financial support, biologists would very shortly eradicate house-flies, idiot children, bean-weevils and bed-bugs ; make it possible for anxious parents to have a family of twelve girls at will and keep the working classes alive exclusively on tinned food.

The tremendous development of scientific knowledge

which followed the coming of the machine was a phenomenon whose social consequences could hardly be envisaged by those who put forward the plea for scientific instruction in the earlier part of the nineteenth century. To-day we can look back over the last century and a half on the growth of a form of civilization which owes its special characteristics to the power over nature which scientific knowledge has conferred. It is now possible to realize that an appreciation, not merely of the conclusions of science, but of the experimental temper of scientific reasoning, has become essential to the intelligent orientation of the individual to an environment more and more determined by the creative thought of science. The classical ideal which is compatible with the view that a man may rightly be considered educated and remain ignorant of man's place in the physical universe, as depicted by science for our imaginative reflection, is an arrogant and impertinent pretension which thinking people will soon cease to countenance. Those who have pressed the claims of scientific education have concerned themselves very little with providing a substitute for what the advocates of classical humanism honestly attempted to achieve. The influence of the Utilitarian School of educationists has superseded what might be called the School of Grammatical Palæontology. One result is that education has been made accessible to a much greater proportion of people. There has grown up a generation of educationists who recognize that heaven does not necessarily lie about us in our infancy. On all sides we see the determination that children shall enjoy school. We now have in our midst the Aimiably Maternal School. There is a danger that the Aimiably Maternal educationists will encourage children to regard childhood as an end in itself; but it is to them that we

must look for the development of the æsthetic side of education. They are replacing the cultivation of "good taste" by the aim of self-realization. They are giving *privacy* its proper place in the theory of education. The domain of publicity—the task of emphasizing the cultural importance of science—still lies with the future. There are, it seems to me, two outstanding pre-requisites for the execution of this task, a recognition of the importance of biological instruction in the school and a closer relation between the teaching of science and of history in the university.

§ 3

A broader conception of the human significance of science will never be achieved until biology occupies a position of greater importance in the school curriculum. Biology contains within its province a point of contact with human life on the one hand, and the methods of an exact and experimental science on the other. Fortunately the educational value of biology is beginning to be recognized. The fact that biology has so recently been added to the school curriculum and that, by no means universally as yet, offers a singular opportunity for educational experiment. It is an opportunity which physics and chemistry, hampered by a heavy load of conservative tradition operating through cut-and-dried syllabuses and stereotyped textbooks, cannot provide. It is an opportunity which carries with it both responsibilities and dangers. The teaching of biology is in one way, and here lies the special opportunity, most fitted to initiate the pupil into the implications of the scientific outlook in human life. Biology handles the kind of matter, living matter, of which human beings are to us the most fascinating, entertaining and familiar varieties. On the

other hand, and in this lies the chief source of danger, biology being a young science with a far greater diversity and complexity of subject matter, is less fitted to demonstrate the essentials of scientific reasoning than a more firmly grounded, older and more exact branch such as physics. We must face our task with a clear recognition of the danger that biological teaching will be made an excuse for supplanting the mental discipline of physics and chemistry by a miscellany of easily memorizable facts which illustrate no conclusions that can properly be dignified by the name of scientific principles. This is certainly what will occur if the Utilitarian school, with its emphasis on where house-flies go in winter, is given full scope in constructing our school syllabuses.

In the early stages of the development of any branch of knowledge there is a period when it is necessary to amass facts indiscriminately, because the significance of particular classes of facts is not as yet apparent. For the study of living organisms in particular an enormous amount of detailed observation was essential before it was possible to formulate the mechanical problems which living matter presents for solution. In such an early stage it is a frequent and fruitful source of misunderstanding to dignify by the name of laws and theories, generalizations which are not scientific principles, but merely mnemonics. Biological textbooks are to this day full of architectural mnemonics. A pertinent instance is the germ layer theory. These have no relation to the generalizations of an exact science such as the Kinetic theory of gases or what is even more modestly called Avogadro's Hypothesis. Biology in our generation has ceased to be merely an encyclopædia of descriptive information. It has to-day attained the status

of an exact and experimental science. As such it is a child of the machine age. The realization of its new status is by no means universal even among biologists. This fact renders the danger to which I have alluded especially formidable.

The birth of the doctrine of organic evolution, before the growth of the modern quantitative study of inheritance and variation, provided it with an experimental basis as a scientific theory, set biologists to the task of tracing hypothetical pedigrees. The amassing of an enormous volume of purely descriptive information acquired the reflected glory of those profound cosmological consequences which the concept of evolution implied. In the latter half of the nineteenth century the study of structure and activity became completely divorced. As a separate subject, focused to a large extent on clinical aspects of its subject matter, physiology branched off independently. Zoology ceased to be the scientific study of living animals, and became the architectural study of corpses and corpses malodorously mutilated in formalin. This development was not without consequences of some purely practical value. Increased knowledge of the life histories of many pests and parasites demonstrated the economic value of biological study to a parsimonious public. Without doubt such studies should continue to receive the financial support that their immense economic utility merits. They should be encouraged as technical developments of biology in the university. Their value is culturally irrelevant to our attitude towards the scope of biological teaching in the school. In the school the scope of biological teaching should be based upon the candid recognition that biology is primarily concerned with the Nature of Life.

This will affect our practice in several ways, which can only be indicated here in very general terms. Elementary textbooks of zoology are written in such a way as to conceal the fact that anatomy was originally an experimental science. Galen had to ligate the ureters to convince the disciples of Erasistratus that the kidneys are the source of the urine. In textbooks of animal biology for beginners it is usual to describe the path of nervous impulse from the skin to the spinal cord and thence to the muscles, as if the reflex arc were something which is evident to inspection. A century and a half of continuous experimental enquiry elapsed between the work of Whyte, who first located the spinal cord as the meeting-place of " sensory " and motor impulses, and that of Waller, who completed the accepted schematization of reflex activity. It is of no educational value to be familiar with a textbook diagram of the reflex arc, unless the experimental evidence on which it is based is clearly understood. The teaching of biology can be as helpful as the teaching of chemistry to illustrate the methods of an experimental science. This implies that the teaching of animal biology must be emancipated from the shackles of the Darwinian tradition of pure morphology. Practical work must include dissection and microscopic observation ; but dissection and microscopic observation must be supplemented by ample demonstrations of an experimental kind.

To a very large extent the construction of our syllabuses will determine the method of presentation. The cultural value of all science teaching is at present hampered by a failure to emphasize the logical development of its subject matter. In the teaching of biology the facts of animal structure should only be presented in so far as they illustrate, and are strictly relevant to, an understanding of the

characteristic properties of living matter. It is still customary in universities to begin the study of the anatomy of the frog by describing its external features. This is a purely architectural attitude to adopt towards an animal. If we must start with the external features of the frog, let us first study some characteristic manifestations of its ever-changing reactivity, such as colour response or mucus secretion, and having defined the experimental conditions which determine these reactions, proceed to examine their structural basis in as much detail as is relevant to our purpose. It is essential that continuity of theme should be developed in relation to a consideration of the organism as a dynamic system.

The teaching of biology for its cultural value also implies the need for the fullest co-ordination with the teaching of chemistry and physics. As every educationist will agree, this is desirable not only from the cultural standpoint, but to get the best practical results. Here there should be no difficulty for the scientifically trained teacher. Such simple demonstrations as the experiments of Lavoisier and Priestley on respiration, or of the action of a digestive ferment, will reinforce the teaching of chemistry and may even quicken an interest in chemistry and physics, where it had not existed before. If an elementary introduction to Mendel's laws illustrated on a comparatively inexpensive scale with poultry be included in the later stages of a school course, the teacher could take the opportunity of experimentally demonstrating the elementary laws of probability. This would provide a helpful introduction to a branch of algebra which in my opinion is relegated to an unnecessarily and regrettably late stage in mathematical education. Being the youngest born, biology in the schools is the Cinder-

ella of the sciences. Some of our headmasters appear to think that anyone is good enough to teach it. It is obvious that desirable results will not be accomplished, if biology is taught by teachers with an exclusive training in descriptive biology unfortified by the study of physics and chemistry.

The æsthetic satisfaction derived from contemplating Man's place in Nature will itself endow the study of biology with cultural value for a few people. But as a school subject biology can make a more general appeal to consideration as the basis of a new humanism. Few things in human life, if any, are the source of more universal inconvenience than sex. The difficulty of satisfying our appetite for food does not present any special difficulty so long as society provides us with the opportunity for work with adequate remuneration. In the domain of sex the difficulty of accommodating physiological necessity to social convenience extends to all ranks of society. In the nursery rhymes of childhood, in the fiction on which our adolescence is fed we are accustomed to romantic expectations which permanently unfit us for the realities of sexual experience. In adult life religious teaching and the legal code reinforce the magical view of sexual behaviour. Even among educated people few possess a secular vocabulary with which to discuss sex intelligently. It would be better for a child never to have heard of Plato than to reach puberty without a scientific knowledge of the nature of sex. If the introduction of biological instruction into the school sweeps away the holistic idea of romantic love, and helps us to envisage the difficult problem of congenial mating as a complex of diverse and separable issues, it will achieve the greatest reform which has hitherto been made in the educational process.

§ 4

Science will not occupy its proper place in cultural education so long as the scientific man himself is a man of narrow intellectual interests. In the university the task of educating a scientifically trained student with a broader outlook than we are accustomed to expect must begin in the way we teach science. To a minor extent this will be encouraged by breaking up our existing -ologies into smaller units. A biologist should not be prevented from studying physical chemistry to an advanced stage because he has neither time nor inclination to devote to a tedious routine of analysis devised for those who are going to take positions in dye-works. A physicist should be permitted to know something about the nature of biological enquiry without wasting half a year cutting hand-razor sections of stems and learning the names of pressed flowers, fish-bones and beetles. But the real romance of science, the realization of scientific understanding as a great mental adventure, will only be achieved when our teaching of science is brought into much closer relation with the study of history.

There are several good reasons why the historical background of a scientific problem should always be brought into sharp relief in university and for that matter in school teaching. The student of mathematics who in the course of a single introductory lecture on the calculus completes the differentiation of the function x^n might be encouraged by the knowledge that he has covered in an hour a problem which took the generation of Barrow, Newton and Leibniz about forty years to solve. I venture to think that the introduction of a little history would make the first steps to algebraic symbolism more interesting at the school stage.

At present it is usual to teach one subject algebra, in which certain conventions are laid down like the rules of bridge, and another subject geometry, in which the pupil learns at a comparatively advanced stage that the proposition $a(b+c) = ab+ac$ physically corresponds to a statement about the addition of areas of rectangular figures. The child is rarely, if ever, told how, from the discussions of such geometrical problems, the Hindu rhetorical algebraists of the first six centuries A.D. were led to deduce certain rules governing the properties of numbers, and how subsequently the Arabs simplified these rules by the development of a symbolic shorthand. Most elementary textbooks of physics contain *ad hoc* proofs of certain verifiable consequences of the inverse square law in electrostatics and magnetism. Why anyone should ever have attempted to investigate the applicability of the inverse square law to electrostatic and magnetic attractions is rarely divulged. A little information about Newton's interpretation of Kepler's laws and the development of the theory of gravitational attractions subsequent to Newton's work would suffice to show how natural it was that Cavendish, Coulomb and Gauss should test out the inverse square law in electricity and magnetism before exploring other possibilities.

Sometimes for an entirely different reason a knowledge of scientific history will assist the teacher to a clearer exposition. The logical technique initially employed in elaborating new scientific generalizations is often capable of a much greater measure of simplification. The teacher who understands the history of his own branch of science will be more likely to realize this. It is generally held that electricity is a suitable branch of science to teach at the school stage. Electricity is much more directly related to the interests

of the average boy than any other branch of physical science. In everyday life it is the phenomena of current electricity which we encounter chiefly. Historically, current electricity was not subjected to exact treatment till the phenomena of electrostatic attraction and of magnetism had been considerably elaborated with the aid of mathematical conventions drawn at first from the theory of gravitational attraction and later from the study of hydrodynamics. It thus happened that Ampère defined the unit of current in terms of magnetic potential. For this reason textbooks of physics usually introduce current electricity after a preliminary treatment of frictional electricity and magnetism. To-day the international unit of current is based on electrolysis. The chemical definition of current involves nothing more than the use of simple proportion. Given a generator and a chemical unit of current the definition of resistance follows empirically from studying changes in the dimensions and materials of the circuit. The idea of electromotive force can be deduced by studying the effect of changing the generator or tapping off current from different parts of a fixed circuit. Ohm's law then emerges self-evidently in the course of the enquiry. At no point is it necessary to introduce difficult ideas imported from magnetism and beyond the range of the pupil's mathematical knowledge. In testing out Ohm's law, in measuring electromotive force or resistance, the galvanometer is only used as a null-point instrument. For an intelligent grasp of the meaning of current, potential and resistance it is therefore only requisite to know that magnets exist and that a suspended magnet is deflected in the neighbourhood of a current. Examination syllabuses are customarily constructed on the assumption that it is impossible to teach current electricity without

first teaching frictional electricity and magnetism. There is no good reason why frictional electricity and magnetism should be introduced at an early stage. The only reason why the fundamental ideas of current electricity are made to appear so formidable is to be found in the history of the subject. The teacher who knows the history of his subject thoroughly will be more likely to realize this.

The teaching of current electricity illustrates the possibility of co-ordination in science teaching in the school. Faraday's laws of electrolysis are generally demonstrated at a fairly early stage in the teaching of chemistry. At this point the definition of the electric current and its measurement is most appropriately introduced. In a school where biology is taught a physical model will prove valuable to demonstrate the effect of fluid friction on the flow of liquids in explaining why blood spurts from an artery and trickles from a vein. By what Dr Wrinch calls the principle of true analogy the same mode may be used to illustrate the ideas of potential, current and resistance.

But the fundamental importance of the historical method in science teaching lies in the fact that no perspective of the relative significance of different types of scientific hypothesis, and no realization of the intellectual potentialities inherent in a scientific generalization, can be obtained without a knowledge of the kind of intellectual difficulties that new scientific ideas met with when they were first formulated. Half-hearted attempts are made to introduce historical information into scientific textbooks. They usually lay more emphasis on the outstanding contributions of individual men of genius than upon the development of ideas. If historical information is only used as a means of promoting ancestor worship, it does more harm than

good. Histories of science are not invariably written by men who have a clear perspective of the general intellectual, and it might be added economic, tendencies of the periods with which they are dealing. For that reason they fail to inspire a critical and enquiring attitude in the reader. Scientific enquiry is essentially progressive. Yet scientific study does not invariably produce a progressive intellectual outlook. It is, I believe, because so few who study science attempt to envisage the generalizations of their subject in their historical perspective, that the product of a scientific type of education is often a more conservative type than the historian or even the classical humanist.

A knowledge of the historical background of science is a necessary prerequisite to an apprehension of science as an intellectual adventure and a challenge to traditional ideas. To possess such an historical background necessitates a knowledge of human history as well as a knowledge of science. The creation of a new humanism based on the claims of natural science is a task which will require a reorientation of historical and scientific studies throughout the educational system. In the school this task is being simplified by the revolt against a tradition which laid too much emphasis on purely national issues. Teachers of history are ceasing to believe that children should be taught to draw maps of the battles of Oudenarde and Malplaquet. The history of ideas is beginning to assume more prominence than the technicalities of military strategy.

In a few universities departments have been founded with the aim of studying the history of science. There are at present all too few scientists who like Dr Singer are capable of promoting a deeper knowledge of the progress of science. The history of science is not a history of pure deduction.

It is not a meaningless incident that Leeuwenhoek, a Hollander, used pepper suspensions to make the first cultures of micro-organisms. It is not a mere coincidence that Leeuwenhoek and Hartsoeker simultaneously discovered the spermatozoon. The first microscope was the signal of a new era in biological science. The invention of the microscope followed shortly after Descartes formulated the laws of refraction of light. It is a task for the historian of science to place this sequence in its proper relation to the interests stimulated by the great navigations, and the struggle for sea-power. A great measure of encouragement to the study of the history of science in the university would, I believe, infuse new ideas into the study of both history and science. It would also re-establish a vital relation between philosophy and science. Instruction in the history of scientific thought could be a nucleus for the synthesis of each fragment of the mosaic of natural knowledge into a coherent picture of the public world as we know it through the medium of scientific enquiry. The philosopher of the future may well be the historian of science.

XII

THE PUBLICIST STANDPOINT AND HOLISM

"I am no coward who would seek in fear
A folk-lore solace or sweet Indian tales:
I know dead men are dead and cannot hear
The singing of a thousand nightingales . . ."
James Elroy Flecker

The history of philosophy has witnessed a succession of makeshifts to accommodate the utilitarian claims of science and the rival demands of what Robert Briffault appropriately calls custom thought and power thought. At every stage in the advance of scientific knowledge new territories have been wrested from the domain of custom thought and incorporated within the legitimate province of scientific method. Each new annexation has called forth some new compromise to meet the requirements of power thought. Scientific hypothesis is ethically neutral. The politician insists that his theories of human conduct must present an aspect of academic plausibility. Darwin's doctrine undermined the complacent dualism which had kept philosophy and natural science in water-tight compartments for centuries. It produced a vigorous revolt against supernatural beliefs. In our generation unbelief has spread to all sections of the community with results that are disquieting to those who pursue the study of philosophy in the hope of rationalizing their social prejudices. It is high time for make-believe to stem the tide of unbelief. Inevitably a new compromise emerges to meet the new situation. Beneath its downy wing holism takes all that mechanistic science can offer to industry and all that statesmanship can cull from metaphysics.

"The time has now come", writes Dr Haldane, "for giving decent burial to the mechanistic theory of life in the same grave with the vitalistic theory". I agree with Dr Haldane in so far as many questions which were contested thirty years ago by those who called themselves mechanists and vitalists have ceased to be regarded as being so fundamental as the contestants imagined or even as being amenable to a final decision. Although the publicist standpoint is admittedly a rehabilitation of the mechanistic theory in the light of those biological developments which have brought into being the behaviourist standpoint in psychology, I have myself preferred to use the term *publicist* for my own point of view. It is rather tiresome to be forced to answer for every misdemeanour of somebody else who happens to call himself a mechanist or a behaviourist. I can therefore sympathize with Dr Haldane's disinclination to accept the vitalistic label for his own beliefs. I differ from Dr Haldane in thinking that we are any nearer to a final reconciliation between a difference of philosophical outlook that arises from the fact that philosophers have different temperaments. If we have outgrown the differences of statement which underlie the controversy between the older vitalistic and mechanistic standpoints, we have not outgrown the differences of temperament which underlie the existence of the two theories.

I have no disposition to state dogmatically the possibility of explaining all the properties of living matter in whatever physico-chemical terms will be employed two thousand years hence. Still less am I willing to be responsible for the billiard ball theory of matter which both Dr Haldane and General Smuts have identified with the mechanistic conception of life. I am content to foresee enormous possi-

bilities for the extension of physical interpretations of the properties of living matter. I fail to see how human knowledge will progress on any logical assumption but that implied by the principle of mechanism in its most general terms. I am not able to accept Dr Haldane's belief that the traditional methods of physiology are useless in discovering the properties of conscious behaviour, though I should hesitate to predict, except in a very tentative way, how far we shall progress in this direction. If I have seemed to exaggerate the possibilities inherent in the future of biological enquiry, my excuse must be that my aim is to stimulate interest in a new philosophical outlook. If there is such a person as the dogmatic mechanist, his views are not what I imply by the publicist standpoint.

When all this has been said, there still exists a very radical difference between the publicist standpoint and the holistic to which Dr Haldane subscribes. That difference lies in the fact that the holist denies the possibility that a certain type of logical procedure is capable of establishing relations between certain realms of experience. Nobody denies that such relations remain at present unascertained. Any dogmatism that comes into the discussion is implicit in the holistic theory. Dr Haldane's position is not merely a rejection of dogmatic materialism. "I am whole-heartedly in agreement", he writes, "with General Smuts in believing that anything which can properly be called scientific physiology is impossible apart from the assumption of what he has called holism".

Of holism as a philosophy of biology enough has been said elsewhere. It contains within it no promise of future progress. Dr Haldane has endeavoured to persuade us that this is not so. I am not able to follow his argument. We

have wasted time, he assures us, in trying to understand the *mechanism* of kidney secretion, when we should really have been striving to find out how the kidneys . . . "engage in their function of keeping normal the diffusion pressures of water and various other non-colloid constituents of the blood." A mechanist, even if we grant that he is misguided and presumptuous in hoping to elucidate the mechanism of secretion, is equally concerned with solving the problem which interests Dr Haldane. The physiologist who sets out to tackle it will proceed in the same way as a mechanist, whether he calls himself one or not. He will not first postulate a wholeness of the non-colloidal constituents of the blood inexplicable in terms of the individual constituents themselves.

The physiologist studies the properties of the muscle-nerve preparation because he believes, rightly or wrongly, that by so doing he will be guided to interpret how muscles and nerves play their respective parts in the behaviour of a whole animal. Dr Haldane himself has devoted years of research to elucidating the properties of hæmoglobin. I presume, he has done so in the hope of throwing light on the way in which the supply of oxygen to the tissues is regulated. Most of his brother physiologists would agree that Dr Haldane's distinguished researches on the physico-chemical properties of a respiratory pigment, which is a very small part of the economy of an animal, do tell us a good deal about what to expect in the behaviour of the organism as a whole. Whatever Dr Haldane may say on the platform he is as good a mechanist as anyone else in the laboratory. Throughout a distinguished career of research he has consistently concentrated his attention upon certain limited parts of organisms. His statement

that physiology is impossible without holism must be taken as a *jeu d'esprit*. So long as biology was dominated by the Aristotelian concept of individuality it remained descriptive. Physiology began when biologists undertook the task of interpreting the behaviour of the organism as a whole by studying methodically the behaviour of its constituent parts. If it be suggested that there is any other physiology, there is no trace of its existence.

Perhaps the appeal of holism is partly due to the curious circumstance that physiologists are notoriously mechanistic about the aspects of physiology they study themselves, and hardly less often vitalistic about aspects of physiology with which they are unfamiliar. The divorce of morphology from experimental biology after the rise of the natural selection theory tended to produce the zoologist who is exclusively preoccupied with the anatomy of dead animals, and the physiologist who is exclusively preoccupied with the casualties of the hospital. In another place I have attempted to show that Dr Haldane could find in modern genetics the clearest evidence that the biologist only progresses when he interprets his data in the same way as the physicist or chemist interprets his. In another branch of physiology, which like genetics lies outside the domain of the clinician on the one hand and the physical chemist on the other, Dr Haldane will not find that the work of Sherrington and Pavlov provides abundant evidence that the holistic attitude is less impotent to suggest new lines of experimentation on reflex action or is more useful in promoting an understanding of the process of learning.

Laying aside the purely biological aspect of the holistic attitude, there are some more general issues which remain

to be discussed. It is never easy for a hostile party to do justice to the standpoint of another school of opinion. I hope therefore that I shall not be accused of drawing a caricature of the holistic theory. To avoid doing so I shall quote freely from the writings of those who support it. Holism, as I understand it, differs from the primitive animism which sees a personal reality behind or within physical objects. It differs from vitalism which sees an essential gulf between the living and the non-living. It differs from the common-sense dualism which invokes Mind as a separate and irreducible concept in dealing with the characteristics of " conscious behaviour ". The difference lies in this, that entirely new properties emerge at *various* levels of existence. *Between* these levels we can operate successfully with the atomistic logic of science, interpreting the properties of a complex system from a study of the properties of its several constituents. *At* these levels we encounter new properties " which ", in the words of General Smuts, " could never have been predicted from a knowledge merely of the parts ". General Smuts mentions under the terms Matter, Life and Mind three principal oases within this desert of uncertainty. Even these do not constitute regions within which the continuous extension of scientific method may be applied successfully. Within the territory of matter " the molecules of water and carbon dioxide are real wholes with new emergent properties ". Thus physics, chemistry, biology and psychology are in Dr Haldane's terminology " independent sciences ".

As far as I am able to see, there is no room for disagreement about how the scientist proceeds within these prescribed territories. Scientific generalizations are attempts to show how the characteristics of complex systems can be

inferred from the properties of their constituent parts. This means, more specifically, when the problem can be reduced to mathematical symbolism, that an equation which defines the four dimensional relations of any system will contain no terms that are not present in some or other of the equations which determine the space-time relations of the constituents of the system. This procedure, admitted Forsyth (1929) in a recent paper delivered to the British Association, " is very largely justified in principle and by results ". He continued to remark, in conformity with the standpoint developed in these essays, that " the question is not that of a division of spheres or levels of existence, some of which are capable of complete explanation on mechanistic principles, while others are incapable . . . for there is no sphere which is not in any degree susceptible of the application to it of the terms and categories of mechanism ".

" Nevertheless ", he contends, " it is gradually coming to be recognized that this procedure gives only a partial explanation of *any* natural process. There is, in any complex process, a principle of synthesis involved, such that, instead of the whole being the mere sum of the parts and being explicable by the mere composition or combination of the parts, it is rather the case that the parts can only be explained by reference to the whole, since they are modified by their relation to it. If so, mechanism must be supplanted, or at least supplemented, by a mode of explanation that gives due regard to this. This principle of wholeness or unity is exemplified in many different spheres of fact, e.g. in atomic structure, in chemical synthesis, in the life of an organism and even in the character of the single life-cell, in the processes of perception and volition, and also in so-called reflex action, in the development of personality and the attainment of social control. Holism, then, signifies that everything in the universe is in some form or another, and in greater or less degree, potentially or actually, an organic whole ; that as anything develops to a fuller realization of its potentialities

> or a fuller perfection of its nature, it becomes more truly such a differentiated and yet unified whole; and that, by implication, the universe itself is an infinite organic whole. . . . This involves that nothing in nature can be explained merely as the result of preceding processes or anterior stages of development. The lower or simpler is the condition without which the appearance of the higher or more complex would be impossible; but the development to higher levels is possible at all only through the impulse to organic unity or synthesis under the controlling influence of the infinite whole."

There exists no difference of opinion concerning the statement that there emerge at different levels of complexity in natural phenomena specific properties which cannot as yet be deduced from a knowledge of simpler systems. Holism seems to imply the further qualification that such properties will never be interpreted in this way. If I am right in my understanding of the holistic standpoint, I am at liberty to leave the onus of proving so dogmatic a conclusion on the shoulders of those who assert it. I am prepared to go further and point out certain difficulties in the way of proving it, difficulties which, in the existing state of knowledge, appear to me to be insuperable. The first is that of explaining why, if physics, chemistry, biology, etc., are intrinsically independent types of enquiry, it happens that there exist such extraordinary similarities in their procedure. The use of the phrase "scientific method" implies that such similarities do in fact exist. Dr Haldane's many distinguished contributions to the advance of physiology might be cited to show how much biology has in common with physics and chemistry. A second difficulty which arises is of a different kind, and illustrates the fundamental similarity of temperament which unites the vitalistic and holistic theories. Scientific investigation is constantly

shifting the levels at which new irreducible concepts must be invoked. This permits us to entertain the suspicion that *emergent* properties are only properties about which we are ignorant. We are under no necessity to regard the present classification of the sciences as anything more than a convenience for arranging time-tables in educational institutions. Before the enunciation of the first law of thermodynamics by Mayer and Joule, heat was a branch of physics quite as separate from mechanics as is chemistry from physics to-day. Organic chemistry to Henry was not, as it is to our generation, the chemistry of the carbon compounds. It was a field in which emerged something " peculiar to animated bodies and superior to and different from the cause which has been called chemical affinity ". In the same year as that in which Henry expressed this view Wöhler succeeded in synthesizing urea. Why should we be so certain that our present classification of " independent " sciences will, like the poor and the Roman Church, be always with us ? To this question I can see no appropriate reply from the holistic point of view. Unless holism can provide us with a clue that will enable us to distinguish whether an impasse in scientific enquiry is due to the imperfection of our knowledge or to the emergence of new properties as " creative " entities, its acceptance could only have the sinister effect of holding back scientific investigation.

On its negative side the holist goes much further from what might be called a centre programme than does the dogmatic mechanist. The principle of mechanism or experimental determinism is compatible with the recognition of different levels of complexity or wholes ; for what is analysable is complex, and what is complex consists of interrelated parts which together constitute a whole. Holism on the

other hand provides us with no explanation of why the principle of mechanism is so astonishingly successful. The logic of science is inadequate according to General Smuts. Dr Haldane asserts that the method of the biologist is different from that of the physicist. We are not told how we can proceed to invent a new and equally successful logic. We are not told in what precise respect the method of the biologist does differ from that of the physicist. On its positive side holism leaves us waiting for a new revelation. Holism is sometimes referred to by its advocates as a category, sometimes as a principle. What the *principle* of holism can do for us remains to be seen. Its popular appeal resides in its promise to restore to ethical values a rational basis. I suspect that this promise is a general election promise.

Though holism does not assume, like the cruder forms of vitalism, a specific élan vital or entelechy, it differs from the less explicit vitalistic theories in detail rather than essentials. In a similar way, the alternative view which I have called publicism is a modern development of the mechanistic conception of life. What I define as the publicist standpoint in philosophy is distinguished from the traditional mechanistic attitude in physiology by extending the limits of verifiable knowledge. To a large extent traditional mechanistic physiology investigated the behaviour of an organism in so far as its behaviour can be predicted, when all synchronous conditions are rigidly controlled. It adopted a frankly agnostic attitude to those characteristics of behaviour in which the antecedent situation is pre-eminently significant to the co-ordination of stimulus and response. The Vitalist school recognized the existence of a definite problem in claiming that a complete solution of the Nature

of Life must take into account the problem of consciousness. They failed to indicate the precise requirements of the problem, and they therefore failed to suggest the requisites for its solution. Their assault upon the mechanistic position collapsed, because they did not state what precise characteristics of the behaviour of living systems are defined by the term consciousness. If the concept of consciousness is to be made clear, it is first necessary to state what properties of the class *living systems* are denoted by its use. This necessitates a definition of the characteristics of conscious behaviour, and an investigation of the way in which it is possible to investigate them. Only when this task is accomplished, can we claim to have undertaken a philosophical exposition of the Nature of Life. Vitalism failed to define the concept of consciousness, because it did not make the necessary distinction between its public and private components. The characteristic of all essentially private statements is the supposition that the means is referable to the end. The characteristic of all essentially public propositions is that the end is considered in connexion with the means. Traditional mechanism disregarded the problem of consciousness. Traditional vitalism assumed that it is impossible to discuss the characteristics of conscious behaviour without introducing teleological implications. The supreme philosophical importance of the work of Pavlov's school lies in the fact that it has inverted the traditional way of looking at the problem of consciousness.

In stating the essential features of the publicist standpoint, I shall indicate first its significance in current biological thought and second its attitude to the scope of philosophy. In its purely biological aspect the publicist standpoint recognizes the folly of stating dogmatically the possibility

of reducing all the properties of living matter to physico-chemical terms. On the other hand it recognizes that biology increasingly makes use of physico-chemical concepts. It also recognizes that we can at present foresee no limit to the successful application of physico-chemical concepts to its subject matter. It recognizes the fact that there is no immediate prospect of reducing the analysis of some of the properties of living matter to the level of pure physics and chemistry. It also recognizes that biology has advanced conspicuously in those regions where the biologist adopts to the subject matter of his enquiry an attitude similar to that of the physicist in approaching the realm of inanimate nature. In particular it sees no necessity for the introduction of teleology into the study of the evolution or behaviour of living beings. It goes further than the older mechanistic outlook in explicitly renouncing the traditional dualism of mind and matter, since it envisages the possibility of indefinitely extending the study of behaviour in terms of reflex action. The publicist standpoint does not assert the possibility of disproving the validity of animistic concepts which have dominated biology in the past, and still persist in a more or less attenuated form. It relies on the increasing measure of success which accompanies the application of quantitative and experimental methods of investigating evolution and behaviour to supersede them. In biology as in physics magical views fall into desuetude, because more profitable ways of dealing with phenomena take their place.

Since philosophy itself is part of the behaviour of a particular organism, the scope of philosophy must, from the publicist standpoint, bear examination in the light of modern biological concepts. In the external world of

modern physics the distinction between *substance* and *form* has been superseded. In the public world of science, i.e. the external world of physics enlarged to take in the subject matter of biology, the distinction between mind and matter does not remain fundamental. What is fundamental is behaviour. This public world is the domain of socialized belief. Whatever cannot be incorporated in its ever-widening territory must remain impenetrable through the medium of discourse. Only propositions that deal with behaviour in its more extended sense have the property of *publicity* or social reality. Reality as a goal of philosophical enquiry is equivocal, since inborn temperament, digestive activity, erotic preoccupations and various other factors decide whether to a given individual the constituents of the public world or of his own private world are more real. Propositions that have publicity are ethically neutral. The hope that philosophy can find a sanction for values is therefore illusory.

§ 2

From the publicist standpoint the business of philosophy is to resolve the problems of human thought into their public and private components. The public component of the problem of consciousness is the analysis of the characteristics of " conscious behaviour ". The private component offers no profitable basis for discussion. Because traditional physiology has failed to recognize that the concept of consciousness has a public component, it has been natural to assume that a mechanistic explanation of consciousness is pure nonsense. Biological progress has annexed the study of conscious behaviour from the province of the private worlds. To the private worlds belong values.

Though differences in values are exaggerated by differences in distribution of wealth and opportunities of education, there is no prospect that we shall reach any general agreement by attempting to rationalize them. In this sense the publicist standpoint is pluralistic. On the other hand the domain of the public world is always encroaching upon the many private worlds. In that sense the publicist standpoint tends towards monism as a limiting case. It offers no short cut to finality. It recognizes the slow accumulation of scientific knowledge as the only road to the gradual socialization of beliefs.

In providing no hope for a rationalization of ethical values, the publicist standpoint comes within the category of the somewhat diverse schools of opinion to which Bertrand Russell refers as the New Realism. If it were possible to prove that a particular set of values is the correct one, a great service could be rendered. People would then be able to argue about human conduct and artistic productions without losing their tempers. I fail to see that holism contains any promise of achieving a consummation so devoutly to be wished. Ethical systems, as Mr Russell observes, are usually found to contain a *non sequitur*. Ethical values represent the private component of social behaviour. Whether the public component will ever come within the scope of physiology remains to be seen. Many thoughtful people have been antagonized towards the mechanistic conception of life, because they associate it with rash and superficial views about human relationships. This is an intelligible but not necessarily a logically justifiable reaction to the naïve self-confidence with which some eugenists, the "histriometers" and other schools of "intelligence" testers have drawn pretentious conclusions from the resources

of immature biological theories and inadequately developed biometrical methods. With such grotesque simplicity did Descartes apply to the physiology of Man the embryo mechanics of his own time. In reality nothing inherent in the most dogmatic assertion of the mechanistic conception of life is logically inconsistent with recognizing the possibility that biology has many new truths to unfold before sociology can securely build its foundations on biological theory, the likelihood that sociology must for long pursue its independent investigation of the natural history of the human species before it is ready to draw extensively on the resources of biology. Only during the first half of the nineteenth century did the concept of Conservation of Energy initiate the era of modern physiology. Only during the latter half of the nineteenth century did physical chemistry begin to elucidate those phenomena of osmosis, colloidal solution and membrane potential now regarded as fundamental for a mechanistic analysis of the living cell. We have therefore no reason to suppose that biologists have elucidated in their own field all those principles which will assist the historian and the anthropologist to advance further in their studies. Arrogant and premature generalizations of individual biologists (not always, or even, I suspect, generally, of the mechanistic persuasion) are regrettable. They do not justify an attitude of distaste for the mechanistic conception of life. I have usually observed that the disposition to interpret the whole field of sociology in narrowly conceived biological terms is associated with a strong antipathy to the physico-chemical interpretation of vital phenomena.

In its inability to rationalize values many people will see a serious limitation of the publicist standpoint. From

the days of the Schoolmen, philosophy has been the " divine science ". It has become so customary to regard philosophy as the handmaid of theology and politics, that Henry Sidgwick and others have refused the use of the term philosophical for a point of view which does not meet this requirement. We justify this by assuming that the fabric of society would dissolve, if we believed that our ethical notions contained no element of finality. It follows that the materialist is to be regarded as a bad man, and scepticism is coupled with immorality, though there is nothing in the root meaning of the word morality to suggest why this should be so. In *Science and the Modern World* Dr Whitehead censures the materialist philosophy of the nineteenth century, because, he declares, it " emphasized the given quantity of material, and thence derivatively the given nature of the environment. It thus operated most unfortunately upon the social conscience of mankind. For it directed almost exclusive attention to the aspect of struggle for existence." [1] I am unable to understand why the political and ethical shortcomings of the nineteenth century should be attributed to the influence of materialistic philosophy. The number of materialistic philosophers was far less than the number of persons who responded to the appeal of the Tractarian Movement. The Lancashire mill-owners who were getting rich quickly were the type of men who attended their Nonconformist conventicles with regularity and subscribed to the London Missionary Society. Against the view that society must necessarily be based on a struggle for the means of subsistence continental Socialism appeared, rightly or wrongly, as a vigorous reaction ; and continental Socialism was the only political

[1] *Science and the Modern World*, pp. 255–6.

movement which can rightly be said to have been explicitly dominated by a materialist philosophy. I cannot discern that there has been a very close historical connexion between people's religious, philosophical and political views on the one hand and their social conduct on the other. It is not easy to explain why the French revolutionaries were anti-clerical and deistic, while the first chartists and pioneers of trade unionism in England were in close touch with the Evangelical Revival. Christianity, with its oriental insistence on the enmity between flesh and spirit and the vanity of worldly riches, would seem little fitted to provide a satisfactory rationalization of the sentiment of western civilization. Yet for long it has done so.

Educational and political institutions grow in response to the exigencies of human demands which are not necessarily influenced by any ethical considerations. This may be seen by considering the case of Mr X, who lives in Balham and has a passion for pineapples. Mr X, not being bothered about metaphysics, is no more disposed than the majority of his fellow-citizens in Balham to assert any final or transcendental sanction for his passion for pineapples. He accepts it as part of his modest unassuming existence. This does not prevent his writing a letter to *The Daily Mail*, when an import tax on pineapples makes it impossible for him to indulge his predilection for the fruit without forgoing other cultural amenities. Meanwhile Mr Y, who lives in Tooting and nevertheless cherishes an earnest desire for more and better pineapples, has written to *The Daily Herald*. Mr X and Mr Y are now joined by Mr Z of Whitby, whose letter appears in *The Yorkshire Post*. By this time the Press cannot resist the social pressure that is brought to bear on the pineapple policy of the party in power. The evening

papers announce in headlines "Nation-wide movement against Pineapple Protection". Mrs J of Houndsditch is pestering Carmelite House with correspondence on the potato shortage, and Miss C of Cheltenham is writing to the Chancellor of the Exchequer about the rising price of prunes. In official circles the determination to do something which will distract attention from prunes, pineapples and potatoes takes shape. If the pressure is firm enough the tax on pineapples and prunes goes, and a promise to give the fullest consideration to the potato shortage is extracted. Alternatively a popular film-star is knighted, and the Press, equipped with better copy, announces that this correspondence must cease forthwith.

There is a difference between a predilection for pineapples and the pursuit of those amenities which are referred to by more abstract terms such as *justice*. Pineapples are less frequently a matter of life and death to those who eat them. Men and women rarely go to the stake or march to battle for the sake of more and better pineapples. They will fight for bread, and a roof over their heads and an eight-hour day. Concerning those forms of social activity which bring us most into conflict with our fellow-beings we are prone to demand some final or transcendental sanction for action. Social co-operation is perfectly realizable without recourse to any such extravagant claims. It is permissible for a professional philosopher to doubt whether there would really be more misery in the world if the ethical convictions of human beings were more lax.

In stating this possibility, I am not endeavouring to give the impression that I wish to be regarded as a "rational" person. If I aspire to a more rational outlook than that of some of the writers whose standpoint I have criticized,

it is not because I have no private world of my own, but because I hope that I am more successful in separating my public beliefs from my private sentiments. I do not pretend that I have no ethical prejudices. My ethical prejudices are as strong as those of most vitalistic philosophers with whom I am acquainted. Often it happens that my prejudices about conduct are not the same as theirs. Where we differ I see absolutely no prospect of arriving at agreement through the medium of argument. I doubt whether I could ever agree with Dr Haldane about a just remuneration for the miner. I accept without hesitation Dr Haldane's authoritative views on miner's silicosis. There is a world of private values which I do not and can never share with Dr Haldane. I have no awareness of anything in the universe which is anything like the Deity of Dr Haldane's Gifford Lectures. His arguments leave me as unconvinced as I was before I read them. Dr Haldane's Deity is part of Dr Haldane's private world. In general I agree with Dr Haldane's interpretation of the Dissociation Curve of hæmoglobin. If in any points I do not follow his reasoning, I should probably agree with him on closer examination of his evidence and inferences. Alternatively I might be able to point out objections to some of his conclusions. I believe that he would then examine my objections sympathetically, and, if he could not dispose of them, modify his views. Dr Haldane's Dissociation Curve for hæmoglobin is not, like Dr Haldane's Deity, part of Dr Haldane's private world. It has become part of the public world.

In my own private world the pursuit of intellectual honesty is a matter to which I attach a good deal of importance. I cannot reconcile with my notion of intellectual honesty a confusion between the ethically neutral con-

stituents of the public world and my private convictions about conduct. When I contend that a rational philosophy must be ethically neutral, I do not pretend that I have no private world of my own. I leave open the possibility that the way in which people come to adopt different philosophical views may one day be made the subject of public discussion as a problem of human behaviour. In my own private world a sense of responsibility to other human beings impels me to refuse to let people think that I am speaking in my capacity as an expert, when I am really giving vent to my sentiments as a private citizen. I do not regard my own private world as unimportant to myself. I choose my immediate friends mainly on the basis of preferences which belong to it. I am unable to understand the disposition to confuse the two issues. It is quite possible to separate them in practice. Sir Charles Sherrington has written an authoritative work on the central nervous system, the importance of which is a public issue. He has also published a volume of verse. In my private capacity I happen to like it very much. I will go further and express the opinion that highly creative work in science, such as Sherrington's, is not unusually associated with an intense system of private values. I admire Professor Sherrington because of the versatility that makes him both a scientist and a poet. But I admire him still more because he has not enclosed the *Integrative Action of the Central Nervous System* and the *Assaying of Brabantius* in one cover as a volume of Gifford Lectures. In publishing them separately, he seems to me to set an example of honesty and modesty which some of his contemporaries might well be persuaded to emulate. William Bateson was a great scientist with very strong political preju-

dices with which I have no private sympathy. Bateson's intellectual honesty was of so fine a calibre that he consistently and publicly refused to associate with Eugenic Reform, lest, he explained, his eminence as a geneticist should appear to give sanction to views in forming which his personal sympathies were likely to override his judgment.

More than a century ago David Hume concluded his essay on *The Academical or Sceptical Philosophy* by contending that "morals and criticism are not so properly objects of the understanding as of taste and sentiment". If philosophy has advanced at all since the time of Hume, I am inclined to think that there is less immediate prospect of making politics a science and more definite information to warrant the hope that the study of morals may one day become part of an experimental analysis of how human beings behave. I can see no reason to suppose that in any other sense will it ever be possible to bring the affirmations of æsthetic and ethical experience into line with scientific beliefs. "If we reason concerning beauty", as Hume observed, "we regard a new fact, to wit the general taste of mankind or some such fact which may be the object of reasoning and enquiry." The rise of anthropology suggests that there are matters of fact which Hume did not actually specify. The substance of his argument remains valid. Science has advanced only when observation has emancipated itself from the affirmations of æsthetic and ethical experience. To set before us the goal which Dr Whitehead proposes may result in hampering science without fertilizing philosophy. Ethics and æsthetics, like politics and religion, are, in Trotter's words, "still too important for knowledge and remain subjects for certitude, that is to say, in them we still prefer the comfort of instinctive belief, because we

have not learnt adequately to value the capacity to foretell". If it was evident to David Hume, it is still more evident to-day that a "rational and modest" philosophy will aim less at providing a formula for complete agreement than at reaching a sensible understanding about matters on which we should be content to differ.

In undertaking to refute Hume's arguments Kant assumed the validity of his conclusions in the method which he adopted. He employed *a priori* principles to establish the existence of a "faculty of pure *a priori* cognition". There were at that time no available materials for a *public* discussion of the problem which he propounded. In our generation a new epoch has been initiated by the physiology of the conditioned reflex. We can now see the correct form in which Kant's problem must be stated, if we are to emerge from the dilemma which arises from the fact that by temperament some philosophers are extroverts and others introverts. It is no longer a question of deciding how we come to know, but how we *learn*. If we are too eager to await the solution of this problem on a purely physiological basis, we have no need to turn, like Kant, to introspective philosophy for an answer. The educational practice of Madame Montessori can throw more light on the origin of the concept of *number* than Kant's discussion of the proposition that seven and five make twelve.

The influence of Christianity in the western world has tended to make the impersonal detachment of science repugnant to most people. We are taught that knowledge puffeth up, but charity edifieth. This, of course, in its own language, expresses the ecclesiastical conviction that human nature is fundamentally sinful. Whatever we choose to regard as good or bad, from the biological standpoint

human nature is neither the one nor the other. Man is a very teachable animal. For that reason it is through intelligent understanding of the springs of human action that the elimination of social discord is most likely to proceed. Those who advocate the religious appeal as the basis of social education have to provide us with an explanation of why the practical implications of revealed dogma rarely receive any recognition before the exigencies of economic necessity compel people to act in conformity to them. No one would deny that religious leaders took a prominent part in the movement for the abolition of chattel slavery. It is also a singular fact that the Protestant Churches entered no protest against the slave-trading activities of Frobisher, Drake and their fellow-heroes of sea warfare. Nor did they disturb themselves with the problem until the rise of the factory system had created conditions which promoted the growth of a different form of labour contract. If war as a means of settling international disputes is abolished in our generation, it is not unlikely that religious apologists will be telling our grandchildren about the prominent part which churchmen took in founding Peace Societies. They will probably be right. War as an institution is becoming so menacing a scourge to civilization that even religious bodies are making themselves active in denouncing it. But if war is to be denounced on the basis of some revealed and final view of human conduct, how are we to explain the fact that a negligible minority of esoteric sects have discovered so significant a conclusion during the past two thousand years of church history? Should we not rather say that the urgency of the modern problem has created a new rationalization? Must we believe that war exists because by nature human

beings are sinful and delight in slaughtering one another? Can we believe that men are so constructed that they can be induced by religious conviction to love their neighbours as themselves? Is it not rather a fact that men are on the whole stupid and indifferent, and that thoughtful people regard war as an intolerable nuisance, but are not as yet clear about how it can be avoided? Is it not to patient study of the ways and means of organizing international government rather than to ethical dogma or religious fervour that we must look for the creation of permanent peace?

From a social point of view I do not think it is a demerit of any philosophy of Life that it provides no pleasant rationalizations as a guide to polite conduct. One would be more interested in discovering some way of ensuring how people can be induced to act consistently with their professions. No religious organization in recent times has succeeded in achieving this result on a large scale. Theology is not entitled to criticize a philosophy because it supplies no basis for ethical values. Theology has failed to show how human beings can be induced to behave in conformity with the ethical values which it imposes on them. For the present I shall eat pineapples in preference to prunes, whether philosophy provides me with a good reason for doing so or not. Men will not be prevented from demanding a living wage because philosophy fails to evolve an ethical theory of the state. A man may be a gourmand without first becoming an Epicurean or a masochist without embracing asceticism as a moral creed. Ethics only lie within the scope of the publicist standpoint in so far as philosophy may indicate the lines along which it is profitable to investigate how people come to articulate certain combinations of speech symbols, and how what they say about their

actions is related to other manifestations of motor activity which they display.

§ 3

I am not unaware of a criticism of the newer mechanistic outlook which has already been made by an anonymous contributor to *Nature* in discussing a symposium at the 1929 meeting of the British Association:

> " The extreme behaviourists or biomechanists, perhaps represented at the conference to judge from reports by Professor Hogben, will of course refuse to take account of any process which does not admit of physico-chemical analysis or description—a position that does not work out well in our daily life and conversation where we have to allow at every turn for intelligent or even rational purpose."

The objection implied in these words is closely akin to a fallacy which is reiterated in most theories of the vitalistic or holistic type. The publicist standpoint does not imply that we do know everything about human behaviour. On the contrary it urges that we know in part and we prophesy in part. I suppose that even the most pessimistic exponent of the vitalistic school would admit the possibility that science will in the course of time modify our habits of conversation in many directions. Everyday conversation always lags behind the advance of scientific knowledge. It would not be difficult to illustrate how frequently our habits of conversation in moments of intellectual laxity are saturated with pre-Copernican and pre-Darwinian views about the universe. It is in no way remarkable that our habits of conversation have as yet failed to accommodate themselves to the advances in biological knowledge that are opening up new fields of investigation into the characteristics of " conscious behaviour ". I am prepared to be told that

I have repeatedly implied traditional views about thought, memory, consciousness and the like in writing these essays. As I bring them to a close I will frankly admit the truth of the charge. The common language which I was brought up to use does not contain the words which would be suitable to a thoroughly consistent development of my present views. I give expression to them, knowing that much which I have written will appear very foolish to those who enjoy the advantage of living two thousand years after I am dead. Had I the ingenuity and astuteness to invent a completely consistent symbolism for the views I have advocated, I could entertain no likelihood that anyone now living would read what I have to say. My own inconsistencies and imperfections do not lead me to infer that human beings will always be forced in their everyday conversation to discuss the problems of human existence with all the limitations to which I am subject.

We are told by Professor Eddington and Dr Haldane that the abstractions of physical science have taken us further and further away from Man, the starting-point of our enquiries. Experimental biology in probing into the traditional distinction between reflex and voluntary activity permits us to recognize that science is in process of taking within the scope of its method fields of intimate human interest. Mechanists of a past generation could not conceive them as capable of annexation. In bringing us back to our starting-point biology has strictly adhered to the method of enquiry which has proved successful in constructing the fruitful abstractions of physics and chemistry. We can thus look forward to a time when the method of science will claim for its field everything which comes within the scope of what people will agree to call knowledge. Because

social activity lies within the realm of what Dr Haldane calls " conscious behaviour " the older mechanistic outlook failed to provide the foundations of a comprehensive interpretation of the Nature of Life. It left Man as the peculiar province of that diffuse type of discussion which draws its sustenance from the abstract noun and comes to fruition as magical gesture. It should occasion no surprise that the new horizon revealed by the growth of biological enquiry now seems contrary to common sense and inconsistent with the language we are accustomed to use in everyday life. Man has existed on this planet for perhaps a matter of five hundred thousand years. During that period little more than five thousand years have been occupied by the building of civilized society. Of that fraction the main development of the essentially social language of science has been compressed to a very large extent within the last five hundred years. We are still the creatures of a tradition of fear, of superstition and of misunderstanding, of childish self-assertion and savage self-submissiveness to magical prohibitions handed down to us from what Professor Levy has aptly called the unsavoury past.

The majority of men are impatient towards the discipline which science imposes upon us. That impatience is a bulwark of magical beliefs. It has been well said by Trotter : " In matters that really interest him, man cannot support the suspense of judgment which science so often has to enjoin. He is too anxious to feel certain to have time to know. So that we see of the sciences, mathematics appearing first, then astronomy, then physics, then chemistry, then biology." Because science does not flatter our self-importance, because science makes stringent demands on our willingness to face uncomfortable views about the universe, because

patience with the slow advance of science requires the effort of intellectual self-renunciation, human nature, deeply rooted in its unsavoury past, is on the side of vitalistic theories. Social privilege is repelled by the mechanistic outlook because of its ethical impartiality. Age brings its impressive authority to reinforce both human nature and social privilege. When the spirit of intellectual adventure dies and with it the courage to face the austere neutrality of a universe which mocks the self-importance of our individual lives, when the ruthlessness of death and decay threatens to rob us of the few circumstances propitious to personal comfort, when the limitations of our greatest achievements are no longer assuaged by the prospect of renewed opportunities, it becomes all too easy to find the formula which provides a compromise for the conflicting claims of magic and science. Perhaps the time will come when our knowledge of the Nature of Life will provide an explanation of this circumstance.